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Clinical Researches on Disease in India. By Charles Morehead, M.D., Principal of Grant Medical College, Professor of the Principles and Practice of Medicine, and of Clinical Medicine; also Surgeon to the Jamsetjee Jejeebhoy Hospital, and formerly Assistant-Surgeon to the European General Hospital at Bombay.—*London*, 1856. 2 vols., 8vo, pp. 687, pp. 766.

The medical works which come under our notice are commonly more or less of two kinds,—one written as introductions to practice, more theoretical than practical, more specious—if the products of clever intellects, than useful, ephemeral in their origin, and as ephemeral in their end; the other, and the more rare, composed with a totally different intent, and of a totally different character, embodying the results of a large and long experience, making additions to our stock of medical knowledge, and becoming—and deservedly—like the works of Hippocrates and Aretæus amongst the ancients, of Sydenham, Laënnec, and may we not say of Bright, amongst the moderns, a permanent portion of the literature of our profession—treasuries of facts constituting the foundations of the philosophy of medical science. To remark that the work which we are about to review belongs to, or even assimilates to the latter class, is certainly bestowing on it a very high compliment; and yet, so much does it display of research, so much of original observation, with other qualities of a high order, that, as we believe, it fully justifies the opinion we have formed of it.

The title portrays well the character of the work. It is essentially practical and clinical, written, as we are informed by its author, after a continued service in India extending over twenty-five years, he during the time enjoying opportunities of observation of varied and great

extent—a field of research such as could hardly have been obtained in any other country, and under circumstances specially favourable for bringing to maturity and giving to the public the results of his experience. Some of these circumstances it may be right to mention, and we shall quote from the preface, not in Dr. Morehead's own words, but in those of Dr. M'Lennan, Physician-General of the Bombay Army, and from a part of a Minute addressed by the latter to his colleagues in the Board of Education, and approved by them and the Government of Bombay, on the occasion of recommending an extension of leave of twelve months at home, expressly for the purpose of preparing this work. Dr. M'Lennan, assigning the grounds for this recommendation, states, that on Dr. Morehead's—

"First arrival in India, he served for two years with European, and for many years with native troops, at different stations. He was then for two years in charge of the sanitary station of Mahableshwur; thereafter, for more than six years resident assistant-surgeon of the European General Hospital, Bombay, an institution in which the inmates are of very varied circumstances as to habits, position in life, nature of duties, and length of residence in India, &c. In that hospital are accommodated the newly-arrived European and the old servant of many years' Indian residence; the seamen of the Royal, Indian, and mercantile navies; the soldiers of all arms and both services, Queen's and Company's; the townsman, mechanic, clerk, male and female, adult and child, from most classes of life and many stations in the interior. The opportunity, therefore, for seeing variety of disease under great diversity of circumstances, is considerable. Dr. Morehead was likewise for six years surgeon of the Byculla Schools. In parts of 1843 and 1844, he was in Scinde, and had an opportunity of observing the state of health of Europeans and natives after the sickly season of 1843. He has been for nearly nine years surgeon of the Jamsetjee Jejeebhoy Hospital, and for six years has been engaged in teaching medicine and clinical medicine in the Grant Medical College; and the records of the clinical wards have been carefully preserved during the whole of this period. He has been twelve years secretary to the Medical and Physical Society, during which time there has been afforded him by the Medical Board the opportunity of becoming acquainted with the tenor of the medical reports and cases from all parts of the Presidency. In 1833, and again in 1853, Dr. Morehead had the opportunity of observing some of the hospitals and medical institutions in Madras, Calcutta, Colombo," &c. &c. (Preface, p. vi.)

After giving a list of the numerous papers contributed by Dr. Morehead, on the Diseases of India, the Physician-General proceeds:—

"Having thus detailed the sources from which Dr. Morehead's experience and fitness for the task which I have ventured to suggest have been derived, I may now add a few words as to the nature of that want which I propose he should supply; and here I honestly give it as my opinion, that till some work of the kind I suggest be brought forth, the efforts of the Indian Governments and their servants in medical education will be incomplete. At present, graduates and students of Indian medical colleges are without any book on practice in Indian disease, as now generally followed, or as requiring modifications to meet peculiarities of native habit and constitution. The duties of the clinical wards in the Grant Medical College have been so carried on and so recorded, as to constitute an important collection of facts and practice, which may be brought to bear on this want. The labour of collecting, digesting, and condensing for such a work will be considerable, and as it is valuable for Indian purposes, it should (it seems to me) receive support and encouragement from the Indian Government, which Dr. Morehead has so zealously and usefully served. I therefore trust my colleagues will support my proposition, and recommend, that after the expiration of

the leave lately granted, Dr. Morehead may have for the above purpose another year in England on Indian allowances, and to count as service, with the right of returning to that place in the Grant Medical College over which he has so beneficially presided." (Preface, p. vii.)

This is a long extract; but, besides its bearing on the work before us, is it not interesting and instructive, as showing the nature of the Indian medical service, and its expansion? The last paragraph we have specially given, both for the excellent intent it displays, and the liberal spirit with which it has been adopted and acted on, to the credit of all concerned. The encouragement which the Indian Government has given to practical science—more particularly to medical science and its diffusion amongst the natives—is deserving of all praise; and how well has it been responded to by the Company's medical officers! What a satisfaction to know that every one of these, if deserving, is sure of reward—of retiring with more than a competency, still in the vigour of life; and if possessed of superior ability, exercised with zeal, sure also of earning, besides fortune, at least local distinction, by becoming either a secretary or member of a directing medical board, or a professor in a medical college, or an official naturalist;—in brief, by filling some appointment of the kind named in one or other of the many departments belonging to a great empire, the business of which, for its efficient performance, requires exact science, and of the kind included in the curriculum of a liberal medical education. Would that we could bestow the same commendation on our Home Government, for the treatment of its army medical officers; a service in which zeal is chilled by want of acknowledgment, merit by want of reward; justifying the remark in the spirited narrative of the siege of Kars, as to "the singular resemblance between the English and the Turks in their approbation [neglect] of military surgeons," and which, in both services, has never been more strongly displayed than in the war just concluded.

Now to our task,—the work of Dr. Morehead, which, were it not for the liberality we have been commending, probably would never have been undertaken, and surely not on the ample plan according to which it has been carried out. making it at the same time a record of facts for the augmentation of science and a handbook of practice for medical officers, and not only in our wide Indian empire, but in our colonies also, even more widely extended. We should be unjust to the author were we to withhold his description of it. Referring to the design sketched out by the Physician-General already quoted, he says:

"In performing this duty I have endeavoured to embody my experience in a connected form, and to illustrate my opinions by cases which have passed under my immediate observation and care; while, at the same time, I have not been inattentive to the views of other inquirers.

"My clinical researches have been directed to disease, as occurring both in Europeans and in the natives of India. I have aimed not merely to increase practical knowledge of the diseases usually termed tropical,—as malarious fever, hepatitis, dysentery; but also to show that affections—pneumonia, phthisis pulmonalis, pericarditis, Bright's disease—familiar to European observers, are sufficiently common in India, more particularly in some classes of the native community." (Preface, p. viii.)

In the Introduction he offers some general remarks on the character of

Indian diseases, their predisposing and exciting causes, and their treatment,—remarks well deserving of attention, as founded on experience, and as expressing briefly the principles of the system by which he has been guided, both in his practice and in his reasoning on theoretical views. He holds that the prevailing diseases, except in the instance of the newly-arrived Europeans, are rather asthenic than sthenic, oftener chronic than acute; that their chief predisposing causes are malaria and cachexia, —the latter a word he prefers to the dyscrasia of Rokitansky; that their chief exciting causes are the same malaria, acting with greater intensity than when predisposing, and external heat and internal cold; that in their treatment consistently with their character, the antiphlogistic plan is seldomer required than the tonic and alterative; that when the abstraction of blood as a remedial measure is indicated, local, by cupping or leeches, is preferable to general by venesection.

In these prolegomena we see much to approve and little to dissent from. They all need further research; and we are sure, if properly entered on, according to the most approved methods of investigation, they will richly repay the labours of the scientific physician. Malaria may be taken as an example. The author justly remarks, that all we know of it is by its effects on the animal system. We may say, were it not for these effects, it would be to us a nonentity. On this account, ought we not to be specially careful in admitting its presence and operation, lest we attribute to it what may be more correctly owing to other circumstances? On the same account, ought not our conclusions regarding its properties to be derived from the largest possible induction? Dr. Morehead, we think, has hardly observed this rule. Amongst the propositions (eight in number) which he has laid down respecting malaria, there are two or three which are open to objection. We shall notice only the seventh, in which he adopts the belief that the noxious properties of malaria are lost in passing over a surface of water, even of small extent; and in adopting the conclusion that it is "attracted by, and clings to, the foliage of trees—thus rendering them a focus of the poison, but at the same time, a protection to tracts of country beyond." Facts we could mention, not according with these statements. The east wind, it is too well known, is not rendered harmless by crossing the intervening sea to our shores. No part of Ceylon is more salubrious than that portion of it which is skirted by a belt of the cocoa-nut palm, from a quarter of a mile to one or two in depth; and we were assured many years ago, that the inhabitants of Trincomalee, in the same island, had occasion to repent the cutting down of trees which had afforded a grateful shade round their dwellings, in an increase of malaria fever after their removal, contrary to the expectations on which they had acted.

Concerning atmospheric heat and cold, as agents productive of disease, we have little hesitation in adopting our author's views, especially his inference—and it is an important one—that a few degrees below the mean temperature are more operative as the predisposing and exciting causes of disease, than the highest degree of temperature exceeding the mean which is easily explicable, keeping in mind the susceptibility of the natives as to cold, from their thin clothing, comparatively spare vegetable diet, and their relaxed cutaneous system—circumstances rendering them better

fitted for bearing a high atmospheric temperature; whilst their dark colour (the darker the more they are exposed to the sun) affords protection, in a measure, from the sun's rays acting as radiant heat.

He remarks, in considering the causes of disease, that "one effect of elevated temperature on the animal system, is the less necessity for animal heat; . . . less demand for food, diminished metamorphosis of tissue, and decreased excretion." This we think just; but we are doubtful of the accuracy of his conclusion that, amongst the natives and the acclimatised Europeans living moderately, observing the ordinary rules of health, bilious complaints, implying an action of the liver vicarious of the lungs, are not more prevalent in India than in cooler climates, and, *à fortiori*, in other warm and tropical climates. From our own experience, we are led to the inference that such complaints are even less common in the West than in the East Indies—in the West, where there is a greater uniformity of elevated temperature, and where, were mere temperature concerned, it might be expected that the liver, in its vicarious function, would be more severely tasked. This difference we are disposed to attribute to the difference of diet: that of the East consisting of a larger proportion of low vegetable food, that of the West containing a larger proportion either of animal food or of vegetable food of a higher nutritive quality, and very much less of fatty or oleaginous matter—a matter which enters so largely into the composition of Indian curries—a preparation of food, in its infinite varieties, rarely absent from an East Indian meal. This peculiarity has not been adverted to by the author. It is deserving, we think, and we hope it will have, his attention in his further researches on Indian disease.

Fevers, very properly, are first treated of. They are included under the head of intermittent, remittent, and common ardent continued fever; and are followed by short sketches of the plague of the Levant, of yellow fever, of typhus, typhoid, and relapsing fevers of colder climates,—these introduced as supplementary, not founded, as the preceding, on his own clinical researches, and consequently not affording any new information, mainly given, it would appear, for the sake of comparison and completion.

The importance of the idiopathic fevers of India is denoted by their proportional frequency and fatality:—of the European troops of the Bombay Presidency, 61·3 per cent.; of the Madras, 27·838; and of the Madras native troops, 27·937, of the strength, are, on an average, annually affected with fever; whilst of the total deaths among the European soldiers in the Bombay Presidency, about 23 per cent. are from fever, and among the officers as high as 28·7 per cent. In the native population of the island of Bombay, during five years, the deaths from fever have amounted to 27,212, which is in the ratio of 40·26 per cent. of the total mortality. The liability of the natives to this class of diseases—little inferior to that of Europeans—is remarkable, especially when contrasted with some other races—such as the Malays, and more especially Africans, who may be considered in a manner exempt from malaria influence;—a peculiarity this, we may remark in passing, more deserving the attention of Government than it has yet received, inasmuch as it is capable of being turned to great advantage in malarious districts, at times when their

military occupation may become necessary. In Ceylon, during the rebellion of 1818-19, and in the West Indies on several occasions, striking examples have occurred of a vast saving of life by relieving white by black troops, under the circumstances alluded to; and reflecting on the subject, we cannot but express surprise that neither Malays nor Caffirs (Africans) have been brought into the military service of the East India Company.

The author's account of intermittent fever affords a good example of his method—combining the practical and rational; careful observation of symptoms, and of the effects of the remedial means employed, with a logical and judicious reasoning on both; exemplifying by an ample record of cases, and concluding with statistical returns: a method which he follows more or less throughout the work.

The pathology which he adopts of intermittent fever is—"That in the cold stage, there is a sedative influence exercised by the morbific cause [malaria] on the action of the heart, and a tendency in the blood to move languidly, and be delayed in the capillary system of important internal organs;" but whether acting through the blood on the heart, or intermediately on the nervous system, or in any other way, as matter of speculation in want of adequate data, he wisely declines discussing. Not the least instructive portion of this chapter are his remarks on the influence of the disease and of malaria—of the frequent recurrences of the one and of continued exposure to the other—in producing "a cachectic state of the system, in which the nutritive processes of the tissues and of the blood are defective and perverted, and in which splenic and hepatic enlargement, and other local congestions of blood, tend to occur"—often leading to death by asthenia—i.e., functional exhaustion; and often, even oftener, to fatal bowel complaints, under the influence of cold acting on the mucous membrane of the intestinal canal.

The treatment he advocates is founded, we think, on just principles—being temporizing, expectant, and palliative, chiefly during the cold, hot, and sweating stages; and active only during the intermission, and then trusting mainly to quinine.

Speaking of the palliative means which he recommends in the several stages of the paroxysm, he makes the following excellent remarks:

"They do not aim at checking or materially cutting short these stages. We are not acquainted with any means that possess this power, but we must rest satisfied with an endeavour to control somewhat the deranged actions. We must be particularly careful in preserving the strength of the patient, not by the injudicious use of food which the system cannot assimilate, but by guarding against a too evacuant and depressing course of treatment.

"The excessive and injudicious use of bloodletting, of emetics, purgatives, mercurials, antimonials, is not only prejudicial by favouring the development of a cachectic state, but it also distinctly favours the recurrence of the paroxysm, and the protraction of the disease. Nor is it difficult to explain this. The malarious influence affects with greater severity, and clings with greater tenacity to, debilitated constitutions. It matters not whether the debility has been induced by medical treatment or by other causes. Under an increasing asthenia, tertians may be observed to become quotidian, and quotidians to become remittent; and I am satisfied that this unfavourable course has not unfrequently been occasioned by the increasing asthenia caused by too depressing a treatment." (Vol. i. p. 40.)

Adding ingenuously, in a foot-note:

"I write with this confidence, not merely from the negative evidence of success attending the opposite course of treatment, but from the positive evidence of having witnessed the evils I describe. I have before me cases, to be afterwards quoted, of my early practice in India, which prove these truths, and show that then they were not familiar to me."

Concerning the active treatment during the period of intermission, by the disulphate of quinine, the following is equally worthy of attention:

"The very earliest intermission should be taken advantage of, and quinine be at once exhibited. The best mode of using this remedy is to give it in doses, from four to ten grains, more or less frequently, according to the severity and obstinacy of the case. The nearer it is given to the period of the expected accession, the more efficacious it will prove to be. For example, if we are acquainted with the probable period of accession, the quinine may be commenced four hours before, and be repeated every second hour. Thus the third dose will fall to be given about the time of commencement of the expected paroxysm; then the quinine should be continued, in perhaps decreasing doses and lengthening intervals, for four or six hours after the period. If the paroxysm has been prevented, the quinine is to be resumed on the following day, in the same manner, and repeated on the third and fourth succeeding ones, but in decreasing doses and at longer intervals after the second or third day. If the type has been tertian, quinine may be given in smaller quantity on the intermediate day than on that of the expected recurrence." (Vol. i. p. 41.)

The dose of the disulphate which he prefers is from four to six grains, seldom using ten, and never to the extent to produce cinchonism, which he considers unnecessary. On the other antiperiodic medicines he places little reliance—such as the liquor arsenicalis, which, in most of his trials of it, he found little if at all efficacious; such as the sulphate of bibeerine, muriate of narcotine, a strong infusion of chiretta, and scruple doses of the Cæsalpinia Bonducella.

We must pass over much that is valuable respecting intermittents and their complications with enlargement of the spleen and liver, with affections of the stomach and bowels, with cerebral disease, and others—such as bronchitis, pneumonia, rheumatism, scorbutus, pericarditis, asthma. Each has a separate section. They will all amply repay a careful perusal.

To select a single example, we shall give Dr. Morehead's view of the pathology and treatment of that complication which is most frequent—viz., enlargement of the spleen, a complication which, out of 243 clinical cases of intermittent fever, he found present in 91.

"With few exceptions," he states, "it is met with only in individuals who have suffered from recurring attacks of intermittent or remittent fever, or who, not having suffered from distinct attacks of fever, have long resided in malarious localities. Under both circumstances, the splenic enlargement is accompanied by a cachectic state of the system, a deteriorated condition of the blood." (Vol. i. p. 58.)

This state, this condition, he ingeniously illustrates by reference to the physiology of the organ and its presumed functions. The following are his indications of treatment, founded on experience, and, as he thinks, in accordance with the physiology and pathology as before given:

"1. To prevent the recurrences of intermittent fever, should they still continue to take place. 2. To remove the cachectic state, and improve the condition

of the blood by the use of all means which tend directly to this end, and by avoiding all measures which tend to induce asthenia, or still further impoverish the blood." (Vol. i. p. 63.)

The remedies he trusts to are chiefly quinine and preparations of iron. The abstraction of blood, the employment of mercury and purgatives, he deprecates as more than hazardous, stating the grounds of his belief. Change of air he recommends in obstinate cases, when it can be had recourse to under favourable circumstances, and the avoidance of the dangerous risk of a land journey through a malarious district, deprived of medical aid,—a proceeding that has cost the life of many individuals.

Remittent fever, the most prevalent disease of the East Indies, as, indeed, it is of the West, and of the South of Europe, and the western coast of Africa,—occasioning the greatest mortality, the most perplexing and difficult to treat in its many varieties and complications, naturally has the special care of the author; and he describes it with no ordinary ability. No part of the work marks better his fitness for the undertaking he has engaged in, or will better repay a careful study.

He attributes remittent fever to the same morbific cause as intermittent —viz., malaria, either acting with greater virulence, or on individuals more predisposed. He considers it, too, identical in nature, differing from intermittent chiefly in degree. The treatment, also, which it requires, he holds to be very similar, and resting on the same principles.

These his views are perhaps as near the truth as the present state of our knowledge can justify. Yet, were we to express our own opinion of the etiology of the disease, we should prefer the conclusion, that the malaria, the materies morbi of the one disease, is rather similar to that of the other, than strictly identical,—the one allied or kindred to the other, like iodine and bromine; and we might extend the inference to the pathology of the two, and their general history. But this is speculative, and of little importance.

We shall not attempt an abstract of the several sections in which the author delineates the disease in its simple form, its varieties, complications, and their pathology. It may be better, limited as we are for space, to confine ourself to the subject of treatment.

Recapitulating the general principles of the medical treatment of intermittents, he explains how, as he thinks, they should be modified to meet the exigencies of the more formidable disease. We shall quote his words:—

"In intermittent fever, there is in general not much risk of injury to important organs during the stage of febrile reaction. Frequent recurrence of the paroxysm is not in general attended with immediate danger to life. It does harm by deteriorating the constitution.

"In remittent fever, on the other hand, there is more commonly risk of injury from the increased vascular action of the stage of exacerbation. Recurrences of the exacerbation are, therefore, attended with immediate danger to life from lesion of important organs or depression of vital actions. Hence, in the treatment of remittent fever, there is more frequently necessity for the reduction of vascular action by depleting means; but at the same time, much greater demand for discrimination and caution, for the evils of the injudicious use of depressant remedies are more immediate, more certain, and more serious. If such are the dangers which more or less attend upon the exacerbation of remittent fever, then the

prevention of its recurrence by the efficient use of quinine given in the remission, is even more urgent than the same indication in the intermission of intermittent fever.

"If it be true, that at some periods of the exacerbation of remittent fever, there may be risk of injury to important organs from excessive vascular action calling for control by depletion, and that, at other periods, there may be danger to life from exhaustion, requiring the prompt use of stimulants and nourishment; if it be also true that the periods of exacerbation and remission are liable to vary in different cases, that it is most important to prevent the exacerbation, and that we possess the means of doing so,—then it follows that there cannot be successful treatment of remittent fever, justice to the sick, or loyalty to the profession of medicine, unless our visits be frequent and our watching attentive and well-timed." (Vol. i. p. 175.)

He corroborates this by contrasting the zymotic continued fevers of colder climates with remittent fever, both depending on a materies in the blood whose power in operation in the febrile action produced we are unable to stop; danger in both, but more in remittent fever than in the others, from excess of vascular excitement; in both danger from depression of vital actions, but with this difference, comparing the one with the other, that in remittent fever

"There are suspensions of the febrile condition, and there is an agent which, effectively used in the remission, tends to prevent the recurrence of the fever, and thus most materially to shorten the general course of the disease. In this (he justly and forcibly observes) lies the strength of medical practice in remittent fever. It has no place in the treatment of the zymotic continued fevers of colder climates." (p. 176.)

In due course he discusses the treatment of remittent fever,—1, in its most tractable and common form; 2, in its severer form, its inflammatory—i.e., when attended with a greater degree of febrile excitement and cerebral and gastric derangement; 3, in its congestive form, connected with a depressed state of the vital actions of the vascular and nervous systems; 4, with a tendency to become continued, and then adynamic in character; 5, with badly-developed symptoms, and symptoms of unexpected collapse. *Seriatim,* also, he discusses its *modus medendi* in its complications,—such as cerebral affection, gastric irritability, jaundice, hepatitis, dysentery,—adding, in a section apart, some general remarks on bloodletting and the mercurial treatment, on cold affusion and wet-sheet packing, on purgatives, emetics, blisters, opiates, quinine, diet, and change of air.

We could wish to point out some valuable observations which occur, relative to the treatment of the disease in its several forms and complications, but our space forbids. We must not, however, pass over altogether the contents of the last-mentioned section, especially as it affords, as it were, the pith and essence of the whole, enhanced by a sound and enlightened criticism on the modes of treating remittent fever which have been in fashion at different times, and have been advocated more or less by distinguished authors. In giving his views, we shall use as much as possible his own words.

Of general bloodletting, he says:—

"In my observations on treatment, I have endeavoured to explain that general bloodletting is an expedient and useful proceeding—sometimes a very necessary

one—in reducing the high vascular excitement of the early exacerbations of remittent fever in sthenic and lately-arrived Europeans, as well as in lesser degrees of excitement, when there co-exist in this state of constitution and stage of fever considerable determinations of blood in organs important to life. These conditions are seldom present except in European troops shortly after their arrival in India. The extent to which bloodletting should be carried in appropriate cases is a point on which the physician must exercise his discretion, keeping in view the ultimate advantage of effecting the advantage aimed at with as little loss of blood as practicable, and recollecting that the judicious removal of sources of irritation, the adoption of free ventilation, the well-timed use of emetics, cold affusion, tepid sponging, and antimonials, are all measures of considerable influence in lowering febrile excitement, and to which it is of very essential consequence assiduously to have recourse, with the view of lessening the necessity of large evacuations. In the treatment of remittent fever in Europeans some time resident in India, and in all classes of the native community, general bloodletting is, with few exceptions, an unnecessary proceeding; and when so, if used, it can hardly fail to be injurious." (Vol. i. p. 198.)

In corroboration of this doctrine, the author states various particulars, derived from his own experience and that of others.

On the use of calomel and the mercurial treatment, which he discusses very carefully and fully, he sums up as follows:

"For these reasons, then, I am of opinion that an endeavour to induce mercurial influence in remittent fever is erroneous in theory and of no value in practice. But the question is not thus easily disposed of. Not only is it erroneous in theory, and of no value in practice, but it is opposed to all rational theory, and very injurious in practice. If it be true that prostration of vital actions and deteriorated condition of the blood are pathological states to be much dreaded in remittent fever; and if mercury deteriorates the blood and favours prostration, on what principle of reasoning can it be supposed that induced mercurial influence can have any other than an injurious effect on remittent fever? I have on several occasions pointed out the tendency of malarious fever to produce a cachectic state of the system, and have endeavoured to enforce the importance of our adding as little as possible to this state of constitution by the remedial means we adopt. To all who, within the last twenty years, have had the opportunity of extensively observing disease in India, in all classes of the European community, the asthenic state, the dyspeptic symptoms, the injured teeth, the pains of sides and loins, the habitually foul tongue, the constipated bowels, the pale alvine evacuations, the depressed spirits, and the sense of sinking at the epigastrium—all clearly traceable to the abuse of mercury—must be familiar facts." (Vol. i. p. 206.)

He adds some valuable information respecting the ascertained effects of calomel on the dog, as ascertained by a series of experiments made by Mr. Murray in 1842, proving, contrary to the earlier and less extended trials made by Sir James Annesley, that its operation, in whatever doses given, is not sedative on any part of the *primæ viæ*, but is altogether irritant. Of cold affusion—that is, using water of a temperature about 80°, he speaks favourably, used timely and with discretion; but not so of the wet-sheet packing, which he considers hazardous, and generally to be avoided. The use of purgatives Dr. Morehead recommends in moderation, given early, and chiefly during the remissions, and in asthenic cases in combination with quinine. After the first two or three days, he deprecates their employment, as irritating the intestinal canal, and disposing to dysentery. Respecting emetics he offers similar cautions.

Blisters he approves, with the intention of controlling local capillary derangement (or, as he sometimes uses the expression, determination of blood), when the stage appropriate for topical bloodletting is passed; the stage of remission he points out as most proper for their application. On the use of opiates he offers, and certainly not needlessly, many cautions:

"I assume (he says) that opium in remittent fever is thought of only when there is restlessness and want of sleep; and that it can be used with safety only in the early stage, when there are not symptoms of marked determination to the brain, and when the pulse is of good volume and soft, and not much above 100." (Vol. i. p. 233.)

He adds other rules, thus concluding:

"Whenever in remittent fever the pulse is towards 120, feeble and compressible; whenever there is wandering delirium, a slight drowsiness, the exhibition of a full opiate is a measure of danger, more particularly towards the close of a febrile exacerbation. In other words, whenever in remittent fever the tendency to death by asthenia or by coma is well marked, a full opiate will expedite the fatal result." (Vol. i. p. 235.)

Respecting quinine, he gives much information, practical and historical, showing how, during the last twenty years, it has been gradually gaining ground in repute and extension of use in India, since juster views of the pathology of the fevers of the country have been formed, and they have ceased to be confounded with, and treated on principles derived from, the practice in the zymotic fevers of colder climates. We could have wished that in his notice of the gradual adoption into practice of this heroic medicine, he had made mention of what had been done in the West, where, as may be seen from the researches of Dr. Blair on yellow fever, it has had a most extensive trial with excellent results. It would appear from documents in the office of the Inspector-General of Hospitals in Barbadoes, which we have had an opportunity of consulting, that quinine was employed even earlier in the West than in the East Indies. It was first used in St. Lucia, in December, 1824, in a case of obstinate intermittent—one that for months had resisted "every medicine that could be thought of," till trial was made of the new remedy, under the action of which it yielded in one day, and without a recurrence.

On diet and change of air, the last topics under the head of general treatment that the author enters upon, his observations are such as might be expected, judiciously cautious. He recommends, as before in the instance of obstinate intermittents, change of air, if necessary, and safely available. He justly remarks that "the importance of placing fever patients, whenever practicable, in a pure and temperate atmosphere cannot be overrated." As to diet, he points out the error of postponing "the use of nutritious food till the signs of prostration are urgently present."

The section following that on the treatment of remittent fever, entitled, On certain Obscure Phenomena probably related to Malaria; and on Adynamic Remittent Fever, infectious in character, observed at Pali and elsewhere, is, as relates to the first subject, very deserving of the attention of the medical inquirer, and especially of members of the profession exercising their calling in malarious districts; and as relates to

the second, it cannot fail to interest those who study the diseases of climate, the habitats of diseases, and appreciate the mystery of their several abiding places. We must pass them over, and also the subjects of the following sections, comprising the common and ardent continued fever of India, the fevers of children in India, and the hospital statistics of fever given in a tabular form; and though unwillingly, we must exercise the same forbearance regarding the contents of the succeeding chapter, relating to Plague of the Levant, Yellow Fever, and Typhus, Typhoid, and the Relapsing Fevers of Colder Climates, to enable us to reserve space for diseases which have come immediately under the observation of the author, and which from their wide spread are of general importance.

Under the head of Eruptive Fevers, the author gives his experience on small-pox, measles, scarlatina, erysipelas, followed by brief mention of hooping-cough and cynanche parotidea. The two latter, it would appear, are of rare occurrence, and consequently of little importance. The same remark applies to scarlatina and erysipelas; the latter, as an idiopathic affection, the former, also rare and even of doubtful existence in its genuine form, that is, identical with the scarlatina simplex, anginosa, and maligna of European countries. Small-pox and measles are better known in India, and on these the author gives fuller information. From his statements, according with the statements of others, the former disease, it may be inferred, is not less dangerous than in cooler climates, and is as effectually guarded against by vaccination, the practice of which hitherto has been but little enforced. Measles, as described by Dr. Morehead, though of less frequent occurrence than in Europe, appears to be as serious in its effects; indeed, the recorded mortality from it in India exceeds that in Europe, in the ratio of about 4·6 to 3 of those attacked Though the details he gives of both these diseases are not without interest, the results of his observations are not sufficiently novel to require to be particularized. It is satisfactory to find that his confidence in vaccination is unshaken, and that, after careful inquiry, he considers it as powerfully preservative against small-pox as inoculation is against a second attack of the disease; which altogether accords with our own belief, formed chiefly in Malta during the epidemic prevalency of the disease in 1830-31. At that time, out of a population of 105,367, as many as 8067 were attacked, and as many as 1172 died, the great majority of whom were unprotected by vaccination; and yet, though there was free communication between our troops and the natives, only 10 of the latter, out of a force of 2219, contracted the disease, and of these 2 only died, one of whom had had small-pox before, and the other had been vaccinated.*
According to the rules of the service, every soldier on his enlistment is vaccinated, unless there be sufficient evidence of his having had small-pox, or of a previous vaccination having taken effect.

On cholera, to which a chapter is devoted, the author gives much valuable information. His account of the disease, which has had his attention for many years, is excellent—truly practical and rational—written with a caution the result of a lengthened experience, and with

* See an account of this epidemic by Dr. John Davy, in his Notes on the Ionian Islands and Malta, vol. vii.

views enlarged by the study of the best authors on the subject. We shall notice briefly some of the more general conclusions at which he has arrived, and nearly in the order in which they occur.

1. He lays it down that the cause of cholera is as yet undetermined, and that the data at present collected are inadequate for the solution of the problem. 2. That in India it has not its seasons of preference. 3. That if the spread of the disease be due to human intercourse, it is very limited indeed. 4. That attention to scrupulous cleanliness and ventilation around the cholera sick, and the placing them wide apart, are, in hospital arrangements, of the first importance. 5. That chilling cold and wet, as in the instance of malarious fevers, are probably determining causes.

Considering the pathology of the disease, he has come to the conclusion that the general and capillary circulation of the blood, and all the actions of the system depending on them, whatever the morbific cause—whether acting first on the blood or on the ganglionic nervous system—are more or less arrested in cholera, with which are in harmony all the morbid appearances that are met with previous to the secondary stage, the stage of reaction, often accompanied by inflammatory action, and in the fatal issues productive of corresponding lesions of tissues.

Under the head of Treatment, the following are some of his results:—1. That when cholera is prevalent, or even apprehended, all cases of diarrhœa should be carefully attended to; and at the same time, great caution should be observed in the use of purgatives, of mercurials, antimonials, or other intestinal irritants. 2. That a simple opiate is the best remedy for the premonitory diarrhœa. 3. That when the cholera discharges are established, opium alone is not to be trusted: the disease then is to be treated on the mild palliative plan. 4. That the administration of acetate of lead and other astringents should not enter into this plan, their effects being doubtful. 5. That general bloodletting is injurious. 6. That the hot bath, emetics, hot saline enemata, rubefacient liniments, saline injections into the veins, the inhalation of vapours, galvanism, cold affusion, and wet sheet, are all either decidedly noxious, or of such a doubtful efficacy as not to warrant their employment.

His recapitulation of the practical conclusions to which he has been led, we shall give in his own words, strongly recommending it to the attention of our readers, and regretting that our limits do not permit us to give a fuller account of this important part of his work.

"These conclusions," he says, "may be shortly re-stated under the following heads:—

"1. In cholera epidemics, there is a proportion of cases ushered in by premonitory diarrhœa, which, if early treated by simple means, are frequently curable, and the cholera attack is prevented. In some instances, however, the diarrhœa is not checked by treatment, and cholera becomes developed.

"2. Cases of cholera occur—common in the early Indian epidemics, but more in the later ones—in which the state of collapse is moderate in degree. In these the tendency is to recovery, not to death; but restoration is materially favoured by judicious, moderate medical treatment.

"3. When collapse is considerable, then we have a condition somewhat analogous to the cold stage of ague, or the initiatory fever of small-pox—a state

which cannot be checked, but which must run a certain course, varying in intensity and duration in different instances, and in which all that we can pretend to attempt is to place the patient in circumstances as favourable as possible for enabling the system to outlive this stage of the disease, while we at the same time carefully abstain from the use of means which may be injurious, not only then, but in subsequent stages of the attack.

"4. When reaction from collapse is taking place, the restoration of the various functions is a slow process, requiring careful watching, mild assistance, and avoidance of officious interference. This expectant course is more certainly the correct one when the stage of collapse has not exceeded eight hours. When the stage of collapse has been longer, the probability of secondary danger is increased; and when this arises it must be met, or when it threatens it may be modified, by cautious, judicious medical treatment, directed with the fact constantly before us, that in this state of the disease, gastro-enteritis is readily excited.

"5. The secondary dangers of cholera are to be treated on general principles, with that care and caution which it is always necessary to observe in all forms of disease present in states of constitution which tend to be adynamic.

"6. In a disease amenable in its milder degrees to ordinary medical treatment —and in its severer ones, though beyond the influence of medicines, still often recovered from—the value of remedies cannot be tested by statistical data, as hitherto recorded. Therapeutic principles drawn from such a source are very likely to be erroneous.

"7. It is to be feared that cholera—as some other zymotic diseases in their severer forms, for example, plague, yellow fever, small-pox—will, in its severer forms, always prove to be little under the control of medical treatment; and that, therefore, in it as in these others, the chief hope of lessening the mortality which it produces, rests on our being able to understand its causes, and to prevent their action. To these important objects, the attention of the medical profession should be earnestly given." (Vol. i. p. 432.)

Dysentery, which the author next treats of,—defining it an "inflammation more or less extensive, more or less acute, of one or other or all of the constituent parts of the mucous membrane of the large intestine,"—is certainly one of the most important diseases of India; and, if we estimate it by its fatal effects, in importance not inferior to fevers. This is shown by the following table:

	Ratio per cent. of admissions from dysentery and diarrhœa to strength.	Ratio per cent. of deaths from dysentery and diarrhœa to treated.	Ratio per cent. of deaths from dysentery and diarrhœa to aggregate mortality.
European troops, Bombay Presidency	22·7	11·100	32·441
European troops, Madras Presidency	23·949	8·721	——
Native troops, Madras Presidency	3·782	12·804	——
Europ. Gen. Hosp., Bombay (dysentery)	——	18·3	24·1
European officers, Bombay Presidency	——	——	5·7
Jamsetjee Jejeebhoy Hospital	——	38·9	21·8
General population of Bombay	——	——	13·50

(Vol. i. p. 439.)

After adverting to the analogy in structure between the mucous membrane and the skin, and the probable analogy of their diseased conditions, he enters on the morbid anatomy of dysentery, which he arranges under the following heads:—

"1. The morbid appearances presented by the mucous membrane of the large intestine.

"2. The complication of inflammation of the mucous membrane of the large intestine, or its results, with peritonitic inflammation, general or partial.

"3. Tumefaction in the region of the cæcum, or sigmoid flexure of the colon.

"4. Displacements of the colon.
"5. Complications of dysentery with morbid lesions of stomach or small intestine.
"6. Complication of ulceration of the large intestine with abscess of the liver.
"7. The co-existence of enlargement of the mesenteric glands with dysentery." (Vol. i. p. 442.)

The above indicates a comprehensive view of the subject, and is well adapted, with the cases introduced by way of illustration, and the author's commentaries on them, to convey a just idea of the many forms of dysentery. So various, indeed, are the morbid appearances, and different, as to justify a preliminary remark which he makes, that, were the lining membrane of the colon as open to inspection as is the outer covering of the body, the diseased affections of the former, now generalized under one name, would probably have assigned them as many distinct names as are employed in the description of the diseases of the skin.

In treating of the etiology of dysentery, he again refers to the analogy above mentioned, taking occasion to express doubt that the disease is ever produced by a specific poison like that which occasions inflammations of the skin—such as the eruptions of small-pox, measles, scarlatina; or such as erysipelas; or some of the squamous, vesicular, and pustular eruptions.

The causes of dysentery he divides into exciting and predisposing. The chief of the former he holds to be cold and wet, under imprudent exposure, acting on constitutions of low capacity for generating animal heat, peculiar to the tropics. The predisposing causes he assigns are more complicated and more obscure: amongst them he ranks highly cachectic states of the system, and malaria. He concludes his discussions on them with the remark, that he prefers considering malaria a predisposing rather than an exciting cause, inasmuch as—

"The cold season of all the hill climates of India will excite dysentery in cachectic individuals, irrespective of the conditions of malaria generation; whereas the view that malaria is itself the exciting cause of dysentery, will tend to condemn all those hill climates in which the conditions of malaria generation are apparent." (Vol. i. p. 533.)

Besides the causes referred to by the author, there is, we believe, another, and which we are rather surprised he has not adverted to—viz., unwholesome water—water containing remains of decomposing animal and vegetable substances, and probably some living organisms, which act as irritants on the large intestine. We have known the disease more or less constantly persisting, so long as the troops in garrison in one of our West India Islands were supplied with water rendered impure in its course; and its ceasing altogether so soon as the same water was kept free from impurities by being conveyed through a well-constructed aqueduct. And it is notorious in the same islands, that the disease is most rife during a period of drought, when from scarcity of water the inhabitants are compelled to use water from ponds and other stagnant collections.

Under the symptoms of the disease, the author does not attempt to specify its several varieties, and assign them names: practically, he considers the following questions more important:

"Is it recent or advanced? Does it engage much or little, and what part, of the mucous membrane of the large intestine? Is it idiopathic, or co-existing with remittent fever? Is it simple, or combined with hepatitis, peritonitis, or other

disease? What is the state of constitution: is it sthenic or phlogistic, or likely to be the subject of erysipelatous inflammation; is it asthenic from former disease, deficient food, or elevated temperature; or is it tainted with malaria, scorbutus, struma, syphilis, mercury, or retained excretions? What is the condition of the mucous membrane: simply reddened, or thickened, or ulcerated, or sloughing?" (Vol. i. p. 553.)

And these are the considerations by which he is guided in the description of the symptoms, for which we must refer to the work itself.

His account of the latency of the disease in its early stage is particularly deserving of the attention of the young military surgeon; as are also his remarks on some other important matters, such as the effects of certain remedies in modifying the symptoms—especially the qualities of the discharges; the impropriety of attaching importance to tenesmus as a diagnostic symptom, inasmuch as it only indicates inflammation or irritation of the lower portion of the rectum; or of considering symptomatic fever a necessary accompaniment of its early stage, or purulent discharge of its chronic stage.

In the treatment of the disease, he proceeds on the principle that "it must vary according to the stage of the inflammation, and the state of the constitution of the individual affected;"—in the early stage, diseased action being to be arrested; in the advanced and ulcerative stage, processes of repair being to be established: the one indicating the use of bloodletting, general and local, mercurial preparations, purgatives, ipecacuanha, and opium; the other, astringents, tonics, alteratives, opium. These several means he discusses at large. We can only notice those conclusions at which he has arrived which are of most importance.

Bloodletting, Dr. Morehead holds, requires to be used with great discrimination, especially general bloodletting; local, by leeches he prefers, excepting in particular cases. Calomel he considers of great service in the early part of the disease, given at bed-time, ten grains with a grain and a half or two grains of ipecacuanha and the same quantity of opium, followed the next morning by from half to an ounce of castor oil, and repeated twice or thrice, according to circumstances; and even continued if the dejections be pale and scanty, the abdomen full, and not much reduction of strength; the indication in view being to excite free secretion from the liver and the small intestine, without aggravating the excited state of the large intestine. He deprecates, and we think justly, the treatment of dysentery by large doses of calomel, on the idea—the illusive idea—of its being a sedative, he believing that in large doses it is commonly the contrary in its effects, and injurious. He is opposed, too, to affecting the system by mercury—the system, when under the influence of mercury, being predisposed to dysenteric attack, particularly in the natives of India. Ipecacuanha he holds in estimation as a dysenteric remedy, and generally applicable either alone or combined with blue pill, or in some cases with opium. With Sir John Pringle, he refers its good effects to its laxative quality. He gives it in doses of from six to three grains, combined with blue pill from five to ten grains, and extract of gentian from four to ten grains, every third, fourth, sixth, or eighth hour, continuing it steadily till amendment takes place. He thinks that the manner in which the combination acts is analogous, but in a less degree, to that of calomel

and purgatives—viz., by maintaining a moderate secretion from the liver and small intestine, favouring the return of the deranged circulation of the large intestine to its normal state. Opium he highly approves, believing it, "in certain combinations and doses," applicable to, and useful in, almost every condition of the disease;—for instance, as given with calomel at the commencement—with ipecacuanha, blue pill, and extract of gentian in the more advanced stages—and alone, or in union with tonics and astringents, after the disease has existed for some time, and is only to be recovered from by a process of repair. He considers the doubts that have been raised against it, and is satisfied that they are not well founded, an opinion in which we cordially agree with him. We have found it indeed an heroic medicine in some of the worst cases of disease—those admitted into hospital from the field during a harassing campaign, in the ulcerative stage; thus, given in one-grain doses every hour, the effect was often excellent, and always so when it exercised no hypnotic influence. Even in these doses, and commencing with them, we did not find that it had any constipating, but rather a laxative, tendency.

We must pass over the other medicines mentioned by the author, though not unworthy of remark. His observations on the treatment of the disease in its chronic state, when associated with cachectic states, whether arising from malaria or a scorbutic diathesis, are specially deserving of attention; as are also the rules which he lays down respecting diet and change of climate. The last, change of climate, is indeed indispensable in obstinate cases, such as resist treatment in India and other hot climates. In such cases, a change to a cooler climate has commonly a wonderful effect, and has been the means of saving life to a great amount amongst our troops. We can speak of this effect from our own experience; and what is remarkable, we have seen men who had been treated with large doses of mercury in India, and this without salivation being produced at the time, becoming severely salivated on gaining strength with improving health at home under a tonic plan of treatment, without taking a single additional particle of mercury. The same men, in their passage round the Cape of Good Hope at an unfavourable season, had become the victims of rheumatism with severe periostitis.

Hepatitis, which follows dysentery, has, as might be expected, the author's careful attention; indeed, no part of his work is more elaborated, more in detail, or enriched more by illustrative cases.

The manner in which he connects the pathology of the disease with the physiology of the organ, is ingenious, novel, and instructive, tempting us to give the passage explanatory of it, though long for an extract:

"Which are the capillary vessels of the liver concerned in the morbid action to which we give the name inflammation? The answer is, I apprehend, sufficiently clear. If the pathological doctrines at present received as to inflammation be correct—viz., that it is an altered state of the nutritive processes of the part affected, depending upon something faulty in one or other of the conditions of normal nutrition,—then the capillaries concerned in inflammation must necessarily be only such as circulate, in their normal state, *arterial* blood for purposes of nutrition. The capillaries of the hepatic artery are the nutrient vessels of the solid structures of the liver, and consequently those alone which can be directly engaged in the inflammatory processes of these structures. On the other hand,

the portal capillaries circulate venous blood for purposes of secretion, and have no concern, as we believe, with the nutritive processes of the organ; they are therefore not *directly* engaged in inflammation. Now this is not a question of mere curiosity. Firstly, if we regard the small capacity of the hepatic artery capillaries in comparison with those of the portal vein, we have, under the view that the former are those concerned in inflammation, an explanation of the fact that the bulk of the organ is little increased, compared with that to which it attains in congestion—a deranged state in which the capacious portal capillaries are directly implicated. Secondly, this view helps to explain how it is that frequently the secreting function of the liver is not deranged in hepatitis. Thirdly, it tends to remove that difficulty which practical writers on hepatitis have more or less experienced in reconciling the results of clinical observation to therapeutic theory. It has been urged that to give mercury with a view to its cholagogue action in hepatitis, is contrary to that general therapeutic principle which teaches that the special stimulants of secreting organs are contra-indicated in the active inflammation of these organs. But this principle—doubtless true when the secreting capillaries and the inflamed capillaries are the same, and carrying arterial blood—is surely without application in the instance of the liver, if we believe that the secreting capillaries and the inflamed capillaries are altogether distinct. Further, if we hold that the hepatic artery capillaries finally communicate with the portal, then to quicken the portal capillary circulation by increasing secretion from its blood seems, in theory, a good way of lessening the stagnation in the capillaries of the hepatic artery." (Vol. i. p. 598.)

The author distinguishes three stages of hepatic inflammation. The first, that of vascular turgescence, with increased redness and some softening: the second, that of exudation, with effusion of lymph into the parenchyma; the third, that of lymph degeneration ("degeneration into pus"), and the formation of hepatic abscess. A good division, and we think unexceptionable; though we cannot go along with him in his pyogenic view, that the pus of the abscess is a mere degeneration of lymph. Call it puritoid matter, such as we know to be derived from the softening of lymph, and may be obtained by slow coction even out of the body, and the objection ceases.

In considering the etiology of the disease, Dr. Morehead refers chiefly to atmospheric influences, vicissitudes of temperature and high atmospheric temperature. He does not even allude to diet and modes of living, which we are of opinion, as already expressed, are not without influence in the production of liver complaints in India. In confirmation, we may refer to Mr. Macnamara's paper on Fatty Degeneration of the Liver, &c., in Bengal, of which a summary has been given in the July number of this Review; and in further confirmation, we may mention that as far as our experience extends, hepatic abscess is of more frequent occurrence amongst troops east of the Cape, when living well in barracks, leading an indolent life, than when in the field, subjected to greater vicissitudes of temperature, undergoing severe fatigue, many privations, and often restricted to a spare and poor diet. We agree with the author that dysentery is hardly to be viewed as one even of the causes of hepatitis, much less as a principal cause in the way promulgated by a late author; and we can refer to the same experience in confirmation of this also, in so much, that in the field, when troops are actively employed, dysentery is commonly exceedingly prevalent, and abscess of the liver is of rare occurrence. The frequent coexistence of the two under ordinary circum-

stances—both of them common complaints in India—is no more, perhaps, than might be expected.

In describing the symptoms of hepatitis, the author offers some excellent remarks, very deserving of attention—especially those tending to show that the disease may exist, and occasionally run into suppuration, without any well-marked symptoms—at least, till low hectic sets in—neither pain of side, nor pain in the right shoulder. nor vitiated biliary secretion, nor enlargement, and other physical signs, the more common attendants on hepatitis, being always and necessarily present.

The treatment described is varied according to the stages of the malady, and is founded on much the same rational principles as the treatment recommended in dysentery. Calomel, Dr. Morehead is decidedly of opinion, should be altogether avoided, as soon as there is any suspicion of the formation of an abscess. We must refer to the work itself for the details. The cautions given respecting the management of hepatic abscess are very judicious.

On the other diseases of the liver, of which cirrhosis and jaundice are the most important, the observations of the author are less extended, and offer less of novelty. The same remark applies to the diseases subsequently treated of—of rare occurrence in India, or rarer there than in colder climates—such as peritonitis, ileus, gastritis, and dyspepsia, Bright's disease of the kidney, diabetes, pneumonia, phthisis pulmonalis, organic disease of the heart and aorta, delirium tremens, cerebral disease, tetanus, blood diseases, comprising pyæmia, leprosy, elephantiasis, scurvy, general dropsy; followed and concluding with an appendix containing articles on the meteorology of Bombay, an account of experiments made with calomel on dogs, and a note on the supposed uses of the bile in the function of digestion.

The account of the diseases just enumerated occupies more than two-thirds of the second volume. Under the head of each, information will be found of much value to Indian practitioners, and not without interest to medical inquirers at home, especially those who are engaged in the study of the influence of climate on the constitution of man, and the morbid tendencies which, in connexion with climate, and diverse modes of living, different races exhibit. Having entered so fully in the analysis of the more important diseases treated of by the author, we must pass over those we have just enumerated, referring—and we do so with confidence—such of our readers who would wish for any information respecting them, to the work itself.

We cannot finally lay down the pen without expressing the satisfaction we have derived, and not only from the matter—the contents of these volumes—but also from the style of their composition,—at once clear, simple, and correct. And we have had a like feeling produced by finding throughout their pages a liberal criticism exercised, or an acknowledgment made of the labours of others in the same field, accompanied by generous notices, and we have no doubt just eulogiums, of professional brethren, especially the deceased, who have contributed to the diffusion and advancement of medical science in India. It is pleasing and refreshing to think of the manner in which this science is exercising a beneficial influence in the Eastern world, not limited to

India, but extending even from the Bosphorus to beyond the Ganges, even to "the Celestial Empire;" and whilst in its immediate action serving the cause of humanity in the divine office of relieving human suffering, in its indirect and reflex action promoting the introduction of the exact sciences, and a humanizing and elevating philosophy. Military glory—a bad sign, if history be true—is at present in the ascendant; let us hope, though it be against hope, that better times will come, when glory of a purer kind will be appreciated,—that due to the real benefactors of their fellow men, connected with the peaceful arts, and nowise associated with desolating, impoverishing, and cruel war, and when it shall no longer be said, *Laudatorem iis dignum esse solum modo tempus.*

Review II.

Transactions of the American Medical Association. Vol. VIII. *Philadelphia*, 1855. 8vo, pp. 763.

The American Medical Association is composed of representatives annually elected by all the Medical Societies, Colleges, and Hospitals in the United States; these representatives, at the expiration of their year of office, becoming permanent members. Its meetings are held annually, at different places. At each annual meeting, individuals and committees are appointed to prepare reports upon scientific subjects specially referred to them, to the best of which prizes are awarded.

The present volume of 'Transactions,' in addition to the minutes of the eighth annual meeting of the Association, the President's address, and list of office-bearers and members, &c., contains ten Reports on different medical subjects, of which the following are the titles:

1. Report on the Diseases of Missouri and Iowa.
2. Report on the Hygrometrical State of the Atmosphere in Various Localities, and its Influence on Health.
3. Deformities after Fractures.
4. Report on the Diet of the Sick.
5. The Pathology, Causes, Symptoms, and Treatment of Scrofula.
6. Report on the Means of Preserving Milk, &c.
7. Report on Dysentery.
8. The Effects of Alcoholic Liquors in Health and Disease.
9. Sketch of the Caustic Pulverizer.
10. Statistics of Placenta Prævia.

Considering the bulk of the volume, and the fact that the American Medical Association is, according to a recent American writer,* the most important of all the medical societies in the United States, we are somewhat disappointed at the small amount of original matter which some of these Reports contain. There is an evident want of original investigation, which is so characteristic at the present time of the Transactions of the leading medical societies in England and on the Continent. To some of the Reports, however, these remarks do not apply; such, for instance,

* Edinburgh Medical Journal, p. 115. August, 1856.

as the first, third, fourth, and tenth. These four papers are of considerable value, and a brief analysis of some of the more important facts contained in them, we propose to lay before our readers.

I. *Report on the Diseases of Missouri and Iowa.*

Missouri and Iowa are two of the most western States in the Union, lying on the right or western bank of the Mississippi, and between 36½° and 43½° north latitude. The principal observations recorded were taken in the town of St. Louis, the capital of Missouri, built on the right bank of the Mississippi, at an elevation of about 400 feet above the level of the Gulf of Mexico. Numerous tables of observations, extending over several years, illustrate its meteorological peculiarities. From these it appears that the annual fall of rain in eighteen years was 42·12 inches, of which 22·86 inches fell in the five months from April to August inclusive. The temperature during the hot months (May to September) might rise to 100° Fahr. in the shade; the mean temperature in July, the hottest month, being 78°; while in December and January, the thermometer often fell to zero, the mean temperature being seldom above freezing point (32°). One of the most remarkable peculiarities of the temperature, was the high range of variation in the course of twenty-four hours, which in the cold months might amount to 40°, and even in the hottest, might reach 25°. The population of St. Louis has increased from 30,000 in 1841, to 127,000 in 1854. The average mortality for eight successive years, before the outbreak of cholera in 1849, and for 1853, in which there was no cholera, was 34·6 per thousand; but during the five cholera years, 1849-50-51-52, and 54, it rose to 59·7, and in 1849 alone was 106·2. The principal diseases noted as producing the mortality, besides the cholera, are diarrhœa, dysentery, intermittent, remittent, typhus, and typhoid fevers, phthisis, and other pulmonary affections. Cholera prevailed principally during the three hottest months, May, June, and July, the largest number of deaths occurring in July. The total number of deaths from cholera, during the five cholera years, amounted to 8380, of which 4317 occurred in 1849 alone. The number of males who died of cholera greatly exceeded the number of females. Of 781 cases in 1851, 488 were males, and 293 females; and of 789 deaths in 1852, 486 were males. Deaths from cholera were also far more numerous in persons above forty-five years of age, than in those under this age; and the "resistance to death" was greatest in subjects from ten to twenty years of age.

The authors of the Report adduce many arguments in support of the contagious nature of cholera. Thus, in 1851, at St. Louis, "as in previous years, the disease first manifested itself among European immigrants, who arrived in the city by New Orleans. Cases of the disease occurred among these immigrants on the steam-boats from that port." (p. 91.) Again, the deaths during the first month all "occurred amongst recently-arrived immigrants." (p. 91.) Similar observations are recorded with regard to the years 1852 (p. 153) and 1854 (p. 225). From St. Louis, cholera was traced into the surrounding districts along the great rivers and most frequented routes. The Report asserts "wherever a much-frequented route is newly opened, thither will the disease march, and thus gain access to detached communities, which, without this means

of communication, might have continued exempt from its ravages." (p. 120.) Numerous proofs of this assertion are adduced. Thus, in the State of Missouri, cholera, during the several years, was "in a great measure confined to the towns on the river banks." In 1849. cholera made its first appearance among the Indian tribes on the Upper Missouri, being imported "by the crowd of emigrants in that year, who opened that route to California." (p. 120.) Again, numerous instances are mentioned of provincial towns in Missouri in which the first cases of cholera occurred in the persons of immigrants from infected districts. (p. 123.)

As regards the localization of the disease in St. Louis, it was found to prevail mostly in badly-drained, badly-ventilated, over-crowded localities; and among those persons who were "imprudent in their habits of living."

A remarkable instance is recorded, showing the length of time during which the formites of cholera may remain in a house. From a certain house

"A family removed, to avoid the pestilential neighbourhood, one of the members having died by cholera. The removed members were exempt from the disease so long as they remained away from the deserted house. In some weeks, their alarm having subsided, and the health of the locality having improved, two of the family returned to the house to supervise its being cleaned, previous to its being again occupied. On their return to their temporary residence, at a distance from their former infected dwelling, both of them were attacked with cholera, one on the night succeeding the visit, the other on the third day after." (p. 232.)

The following statements show the number of deaths in proportion to the number of persons attacked. In 1851, out of 169 cases admitted into the St. Louis Charity Hospital, 89 died, or 52·6 per cent.; and of 128 cases admitted into the City Hospital, 79 died, or 61·7 per cent.

We have no information as to any peculiarities in the mode of treatment.

The whole Report confirms the opinion which is now pretty generally acquiesced in by the profession in England and on the Continent, that cholera *may be* propagated by human intercourse, or in other words, is contagious. Whether contagion be the sole or even the principal means of its propagation, remains to be decided, but that cholera is contagious few will now venture to deny. Many observations made in our own country during the recent epidemics; the admirable Reports by Löberg, Kierulf,* and others, of the cholera in Norway; and the very interesting researches of Dr. Alison of Edinburgh, and Dr. Budd of Bristol, recorded in the first volume of the new 'Edinburgh Medical Journal,'† should suffice to convince the most sceptical. It seems astonishing that in India, the birthplace and head-quarters of the disease, the doctrine of contagion is almost universally repudiated by our professional brethren. "All our experience," say the editors of the 'Indian Annals of Medical Science,' "is opposed to the doctrine of contagion;"‡ and this opinion we know to be the one which in India is generally entertained. This difference of opinion on the part of those who have such ample opportunities of observation, we think may admit of explanation in the fact, that in India all the predisposing causes of cholera are in constant

* See British and Foreign Medico-Chirurgical Review, p. 102. July, 1856.

† pp. 481, 668, 1112. ‡ Vol. i. p. 456. 1853.

operation, more especially prolonged heat, decomposing organic matters, a more or less debilitated state of the constitution, and excitable condition of the nervous system, &c.; and hence, no sooner is cholera imported, than it spreads with such rapidity as to resemble an epidemic invasion. Cases of undoubted contagion, however, are not wanting in India. Mr. Barry, a surgeon in the Bengal service, has recorded an outbreak of cholera which occurred at Gowalparah, in Upper Assam, in 1853.* In this instance, the cholera was evidently imported into a healthy station by a body of Sepoys coming from an infected locality; every case of the disease could be traced to communication with the sick, a large number of attendants on the sick were seized, but those who separated themselves escaped in every instance.†

As regards continued fevers, the two forms pointed out by Louis in France, and Dr. Jenner in this country, are also met with in America. Thus we find:

"In general the term *typhoid* is applied to continued fever when complicated with enteritic lesions, the term *typhus* being retained to nominate that form in which cerebral lesions predominate without enteritic lesions, and in which there occasionally occur implications of the pulmonary organs." (p. 106.)

The following important observation is made in reference to the typhoid form:—"Cutaneous eruptions, either of petechiæ, vibices, or 'taches rouges,' are frequently seen in the disease, but are by no means its invariable accompaniment." (p. 110.)

In many of the cases of continued fever, the abortive treatment by quinine was adopted. The result is contained in the following somewhat unsatisfactory paragraph:

"We have seen the fever successfully subjugated in its early stages by quinine; we have, on the other hand, seen the disease evidently exasperated by it. We have known the quinine prove injurious in the early stage of the disorder, and very promptly efficient in the same cases at a later period."

Under the head of periodic fevers, a remarkable instance is mentioned, in which the *partial* drainage of a lake produced such a hotbed of malarious fever as to render the surrounding district, previously comparatively healthy, uninhabitable; but which complete drainage and desiccation restored to its original condition. (p. 209.)

An epidemic of scarlet fever at St. Louis in 1853 is recorded, remarkable for the large number of cases proving fatal in the early stage. Anasarca was observed to supervene most frequently on the decline of the mild cases. Some observations also by a Dr. Engelman are mentioned, with the object of showing that an epidemic of scarlatina may gradually pass into one of measles, and that there is a transition form, partaking somewhat of the characters of both, but which protects the system from a recurrence only of itself, and not of the other two.

* Indian Annals of Medical Science, vol. i. p. 448. † Ibid.

III. *Deformities after Fractures.* By Frank H. Hamilton, M.D., of Buffalo, New York.

The author premises his paper by observing that, although most hospital reports show the result of the treatment of fractures in as far as they prove fatal or are cured, yet there are no tables which indicate the "value of the cure;" or, in other words, the presence or amount of resulting deformity. He goes on to state that deformities after fractures, even in the hands of the most experienced, are far more frequent than is generally supposed, and that a contrary belief has originated in a want of careful examinations and measurements. Notwithstanding the assertions of Dr. Hamilton that his remarks are applicable to the surgery of other countries as well as of America, and although we have no precise statistics to bring forward to prove the contrary, we are hardly prepared to admit, that on this side of the Atlantic, fractures are so frequently followed by deformity as he asserts. We are reminded on this occasion of a remark which much surprised us, and which serves as a sort of corollary to the above. The recent American writer to whom we have already had occasion to allude, states that general practitioners in that country "are frequently subjected to the annoyance of prosecutions for mal-practice, and most of these have been in cases of fracture." These observations are not made with a view to disparaging Dr. Hamilton's paper, which we consider of great interest. The inquiry which he has instituted is well deserving of being followed up by surgeons in this country; for certainly, if his statements are correct, the art of treating fractures has "not attained that degree of perfection which surgeons have almost universally claimed for it."

Detailed statistics are given in the Report as to the result of treatment of fractures of the ossa nasi, septum narium, superior and inferior maxilla, and clavicle, which fractures all surgeons know to be more or less frequently followed by some deformity. No mention, however, is made of fractures of the bones of the extremities, deformities in which are of far more importance than in other parts of the body, but, as we believe, of much less frequent occurrence. It is to be hoped that this defect will be supplied in a subsequent report.

Of fractures of the nasal bones, 22 cases are mentioned.

In 9 there was deformity, but no treatment.

3 died from severity of other injuries.

Of 10 subjected to treatment, in 7 there was permanent deformity, and in 3 only complete restoration.

The author confirms the observations of Malgaigne as to the extreme rapidity of union of these fractures, repair taking place without any provisional callus.

Seven cases of fracture of the septum narium are mentioned, all of which were followed by deformity. A surgeon was consulted in five of the cases, but no treatment was adopted in any.

Of fractures of the superior maxilla, 6 cases are recorded. In 2, death resulted from the severity of the injuries; in all the remaining 4, more or less deformity remained. One of the principal causes of this deformity was a depression of the malar bone, and the author suggests a

mode of elevating it by means of a screw levator screwed firmly into the bone, after making an incision through the soft parts. In the absence of an instrument for the purpose, he adds, "a joiner's gimlet might answer tolerably well"!! There are few surgeons, we hope, even in America, who would have recourse to such a procedure.

Of fractures of the inferior maxilla, there are 18 cases:—1 died; in 2, result was not known; in 1, fracture not united after seventy days; in 4, permanent deformity, but slight; in 10, no deformity.

These results appear on the whole successful.

Of fractures of the clavicle, there are 14 cases recorded as incomplete, and 39 as complete. Of the former, in 7 the result was perfect, and in 7 there was deformity; and of the latter, there was deformity in 32, and union without deformity only in 7. The amount of displacement varied from a quarter of an inch to an inch, and the inner fragment was almost always found above and in front of the outer. Numerous suggestions are made as to the treatment of this fracture; and a new contrivance, invented by the author, is described.

IV. *On the Diet of the Sick.* By Charles Hooker, M.D., of New Haven, Connecticut.

At the commencement of the Report, several rules are laid down for the diet of the healthy. Various diseased conditions are mentioned as resulting from a too fluid diet, and among others, upwards of 30 cases of purpura hæmorrhagica, "which have been connected with the habitual excessive use of drinks." Some interesting and important observations are made on the relations between scrofula and an oleaginous diet. On this subject, the author's observations for many years have led him to the following conclusions:

"1. Of all persons between the ages of fifteen and twenty-two years, more than one-fifth eat no fat meat.

"2. Of persons at the age of forty-five, all, excepting less than one in fifty, habitually use fat meat.

"3. Of persons who, between the ages of fifteen and twenty-two, avoid fat meat, a few acquire an appetite for it, and live to a good old age, while the great proportion die of phthisis before forty-five.

"4. Of persons dying of phthisis between the ages of fifteen and forty-five, nine-tenths at least have never used fat meat."

These observations confirm the views which are maintained by Dr. Bennett and others, that in phthisis there is a deficiency of the oily ingredients of the tissues, and that hence the most rational treatment consists in the administration of oleaginous ingesta, in such forms as to be most easily assimilated. Dr. Hooker also confirms the observation which has been made in this country, that patients "who have never used fat meat—to whom, indeed, it is absolutely disgusting—will readily take cod-liver oil;" but adds: "the few patients who have phthisis after a habitual use of fat meats, are little, if any, benefited by cod-liver oil."

Hence, in estimating the probable good effects of a cod-liver oil treatment, it would seem advisable to take into consideration the previous habits of the patient, as regards the use of oleaginous food. Several cases are detailed of advanced phthisis, which were cured.

The author insists strongly upon the injurious effects of tobacco in pro-

ducing dyspepsia, maintaining that it acts not only as a narcotic, but also by increasing and wasting the saliva. This statement we readily corroborate. We have ourselves seen several cases of aggravated dyspepsia attributable solely to the excessive use of this weed, and remediable by a removal of the cause.

In the treatment of typhus, Dr. Hooker enforces the injunctions of the late Dr. Graves, of Dublin, to guard against the patient dying of starvation. He recommends that small quantities of solid food, such as panada, dry toast, or even a little meat, along with small doses of quinine, should be repeatedly given. Under this treatment, he lost only 8 out of 195 patients; and adds that, at the Connecticut Hospital, with this mode of dieting, the mortality from typhus is only four per cent. There can be little doubt of the propriety of guarding against starvation in typhus, but the cases alluded to must have been of a mild type. At the London Fever Hospital, where small quantities of nutriment (such as beef-tea, milk, &c.) are repeatedly given, the mortality during the two years 1854 and 1855 was almost twenty-two per cent.

X. *Statistics of Placenta Prævia.* By J. D. Trask, M.D., of New York.

This paper is the most important of the series. To it was awarded the prize of the Association. The author has collected in a tabular form, with great labour and care, 353 cases of placenta prævia, his object being to show the result of the treatment of this complication of labour by the plan recommended by Dr. Simpson, of Edinburgh—viz., the complete separation and extraction of the placenta, as compared with that from the more ordinary treatment by turning.

Most of our readers are no doubt familiar with Dr. Simpson's paper as first published in the 'Edinburgh Monthly Journal' for March, 1845, and reprinted in the first volume of his 'Obstetric Memoirs.' In this paper, Dr. Simpson has shown that, in placenta prævia, the mortality to the mother under all the previous modes of treatment, was 180 out of 654 cases, or 1 in $3\frac{6}{10}$ths; and from turning alone, 1 in $2\frac{9}{10}$ths; whereas out of 141 cases in which the placenta became spontaneously detached, only 3 deaths occurred, or 1 in 47, which could in any way be attributed to the complication in question. He has therefore recommended that, in certain cases of placenta prævia, the placenta should be artificially detached.

Dr. Trask's statistics are, on the whole, confirmatory of Dr. Simpson's; but he points out, and we think with justice, that it is hardly fair to estimate, like Dr. Simpson, the results of *artificial* separation as the same as those of *spontaneous*. The cases which he has collected show a great difference between the two.

Dr. Trask's cases are classified in three tables. The first table includes 251 cases, in 200 of which turning was adopted. Of these 200, 59 died, or 1 in $3\frac{4}{10}$ths.

The second table embraces 36 cases in which the placenta was spontaneously separated and expelled. In 29 the result is mentioned, and out of these there were two deaths, both of which were caused by diarrhœa (one eight, and the other twelve days after delivery), so that here, not a single death could be referred to the placenta prævia.

In the last table are given 66 cases, in which the placenta was artificially separated and extracted. Out of 60 of these cases there were 13 deaths, or 1 in $4\frac{6}{10}$ths. The author, however, adds, that the cases in the last table embrace "a considerably larger proportion of severe cases than are ordinarily met with."

The mortality to the child after artificial separation, as ascertained by Dr. Trask, corresponds with that given by Dr. Simpson. Thus, in Dr. Trask's cases, it was 1 in $1\frac{15}{32}$nds; and in Dr. Simpson's, 1 in $1\frac{35}{78}$ths.

The author concludes by recommending the treatment by artificial separation in similar cases to those in which it was originally proposed by Dr. Simpson.

Review III.

1. *Leçons de Physiologie Expérimentale appliquée à la Médecine, faites au Collège de France.* Cours du Semestre d'hiver 1854–55. Par M. Claude Bernard.—*Paris*, 1855.

Lectures on Experimental Physiology applied to Medicine, delivered at the Collège de France. Winter Session 1854-55. By M. Claude Bernard.

2. *Sur le Méchanisme de la Formation de Sucre dans le Foie.* Par M. Claude Bernard. (In 'Comptes Rendus de l'Acad. des Sciences,' tom. xli. pp. 461-469. No. 13, Sept. 24, 1855; and in 'Annales des Sciences Naturelles,' Quatrième sér., tom. iv. pp. 109–119.)—*Paris*, 1855.

On the Mechanism of the Formation of Sugar in the Liver. By M. Claude Bernard.

3. *Saccharine Matter, its Physiological Relations in the Animal Economy.* By Frederick William Pavy, M.B. (In 'Guy's Hospital Reports.' Second Series. Vol. viii. pp. 319–344.) – *London*, 1853.

4. *Researches on the Nature of the Normal Destruction of Sugar in the Animal System.* By Frederick William Pavy, M D. Lond. (In 'Guy's Hospital Reports.' Third Series. Vol. i. pp. 19–37.)—*London*, 1855; and, in a condensed form, in 'The Proceedings of the Royal Society of London,' for May 3rd, 1855. Vol. vii. pp. 371–376.

5. *Sugar in the Animal Economy in Health and Disease.* The Harveian Society's Prize Essay for the year 1856. By James L. Bryden, M.D. Edinb., H.E.I.C.S. Not yet published.

About two years ago, we devoted a short article to Claude Bernard's contributions to physiology. The most important of his discoveries is unquestionably that of the formation of sugar in the liver, and the 'Physiological Lectures,' which we now propose to notice, are for the most part devoted to this subject, and to its bearings on the physiology of diabetes. They are twenty-five in number, and were delivered in the Collège de France, in the winter of 1854–55. The phrase "experimental medicine," which was adopted many years ago by Magendie (Bernard's predecessor in the chair), expresses, perhaps, better than any other, the nature of the course, which is usually devoted to some one or more subjects which, in the opinion of the Professor, require special experimental elucidation.

Until very recently, it was regarded as an established fact, that the vegetable kingdom alone had the power of forming sugar, and that any sugar found in the blood, urine, &c., of animals, must have had its origin in the amylaceous or saccharine portion of the food. Experiments of unquestionable accuracy have, however, demonstrated that the animal organism has also the power of forming sugar, altogether irrespectively of the nature of the food; that sugar exists normally in the blood, in a certain part of the circulation—namely, from the hepatic veins to the pulmonary capillaries, in both carnivorous and herbivorous animals; and that the quantities of sugar which we find in these two great classes of animals, do not present any sensible differences.

"In man, and in all animals, there is a sugar-producing organ, and this organ is the liver; and as all secreting organs are impregnated with the product of their secretion, as the kidney is impregnated with urine, the testicle with spermatic fluid, the pancreas with the pancreatic juice, and the salivary glands with their different varieties of saliva, so is the liver impregnated with sugar; and it is the only organ of the body which in the normal state presents this peculiarity. To convince ourselves of this, we have only to take the tissue of any freshly-killed animal, to pound it and boil it with a little water, and to search for sugar by the ordinary means in the (filtered) liquid decoction."*

In performing this experiment, it is necessary to make the filtered fluid pass through animal charcoal, in order to decolorise it, and then again to filter it, before we apply the ordinary tests; but this being done, we obtain ready evidence of the presence of glucose or grape-sugar by Trommer's test (the reduction of oxide of copper), by boiling with liquor potassæ, and by fermentation.

The presence of sugar in the liver, and in no other organ of the body, is a fact that has been established by Bernard, by observations on a large number of animals in almost every department of the zoological scale of beings.

In order that the experiments made on man should correspond with those instituted on animals, Bernard was obliged to confine his observations to cases of sudden death in healthy persons. He examined the livers of five executed criminals, of a man who was killed instantaneously by a gun-shot wound, and of a diabetic patient who died suddenly from pulmonary apoplexy.

The following are his results, arranged in a tabular form:

	Age.	Weight of liver in grammes.	Sugar in 100 parts of liver.	Ditto in the whole liver.
Criminal (A)	45	1300	1·79	23 27 grammes.
Criminal (B)	45	1330	Sugar present: quantity not determined.	Not determined.
Criminal (F)	—	1175	Ditto.	Alcohol obtained by fermentation.
Criminal (V)	22	1200	2·142	25 704
Criminal (C)	—	1175	Alcohol obtained by fermentation.	
Gun-shot case	30	1575	1·10	17·10
Diabetic case	—	2500	2·30	57 50

In the first three cases, the person had taken no food since the preceding evening; in the others, digestion was going on. It should further

* Bernard. Leçons, &c., p. 51.

be mentioned, that in the gun-shot case, the liver was not examined for two days after death, and decomposition, which destroys the sugar, had commenced.

With immense labour and unwearying powers of work, he has obtained evidence of the presence of sugar in the liver, throughout almost the whole animal scale. He has found it in mammals (of which at least seventeen different kinds were examined, besides man), in birds (sixteen different kinds), in reptiles (ten different kinds), in osseous fishes (twelve different kinds), in cartilaginous fishes (three different kinds), in molluscs (eight different kinds), and in articulate animals. The relative quantity of sugar in the liver varies little when the system is in a normal condition; it very seldom exceeds 4 per cent., the mean being from 1·5 to 2 per cent. in mammals and birds; while in reptiles, fishes, and molluscs it is somewhat less. It is unnecessary to notice the experiments by which he distinctly proves that the sugar which exists in the liver is identical with that which occurs in diabetic urine; and we pass on to his demonstration that this sugar is secreted in the liver, and that it is in no way connected with the nature of the food.

"The most simple proof seems to be afforded by withholding all amylaceous and saccharine food from an animal, and observing if sugar still continue to exist in the system. This experiment has been made upon a great number of animals (dogs), which we have fed exclusively on flesh for six and even for eight months. When, at the end of that period, the animals have been killed, we have found 1·9 per cent. of sugar in the liver, which is as much as occurs in dogs that have been kept on a mixed diet.

"Birds of prey, owlets, taken in their nests, and fed exclusively on raw bullock's heart for three months, were then killed; their livers always contained sugar in the normal quantity (1·5 per cent.), while the other tissues presented no trace of this substance."*

The above experiments suffice to prove the persistence of sugar when no amylaceous or saccharine matters can by any possibility be introduced into the system; and the case of the young owls completely overthrows the view that has been maintained against Bernard—namely, that the sugar may have been localized and hoarded up in the liver from saccharine food taken during *une alimentation antérieure*. But the main demonstration is based on the relative analyses of the blood of the portal vein as it enters, and the blood of the hepatic veins as they emerge from the liver. A dog, after fasting thirty-six hours, was fed freely with boiled sheep's head, and three hours afterwards, when digestion was in active progress, was instantaneously killed (by division of the medulla oblongata) in the presence of the class. The blood collected from the portal vein, before its entrance into the liver, gave no trace of sugar; while, on the other hand, the blood of the hepatic veins contained a considerable quantity of sugar, as was proved both by Trommer's and the fermentation test. This experiment has been fully confirmed by the comparative analyses of these two kinds of blood, instituted by Professor Lehmann with special reference to the sugar question, and laid before the Académie des Sciences a few weeks after the delivery of this lecture. Lehmann found that the portal blood never contained the least traces of sugar,

* Leçons, p. 69.

either in dogs when fasting, or in dogs living on flesh; but when the were fed on boiled potatoes, the portal blood did contain sugar in suc small quantity that its amount could not be determined. Minut quantities were also found in the portal blood of two horses. The bloo of the hepatic veins, on the other hand, always contained sugar *en forn proportion*, as is shown in the following tabular view of his collectiv observations:

Animal.	Food.	Quantity of sugar in the solid residue	
		Of portal blood.	Of hepatic blood.
Dog	fasting for two days	none	0·746 per cent
Dog	ditto	ditto	0·638 „
Dog	ditto	ditto	0·804 „
Dog	flesh	ditto	0·814 „
Dog	ditto	ditto	0·709 „
Dog	ditto	ditto	0·946 „
Dog	boiled potatoes	traces	0·981 „
Dog	ditto	ditto	0·854 „
Horse	bran, hay, and chopped straw	0·055 per cent.	0·635 „
Horse	ditto	0·052 „	0·983 „

Were it necessary, we might quote memoirs by Lecomte, Moleschott, and others, affording further demonstration of this fact; but Lehmann's analyses appear so decisive, that we shall proceed without delay to the further consideration of Bernard's lectures. We have already alluded to the view held by some of his opponents—that the sugar is merely localized in the liver, just as mercury, copper, and arsenic are found in that organ long after the administration of the salts of those metals. He now proceeds to demolish this objection by the following experimental proof. It has been already shown that young birds of prey fed exclusively on flesh from the period of their hatching, contain from 1·0 to 1·5 per cent. of sugar in the liver; and the presence of this constituent in the liver of the unhatched chick is very readily demonstrated. If we perform similar experiments on the mammalian fœtus, we arrive at the singular conclusion, that the "glycogenic function only commences at a special period of intra-uterine life, and that the saccharine matter augments in proportion as the animal approaches the time of birth." Bernard exhibited to his class a fœtal calf at four or five months, in which the liver presented sugar, while in the liver of a corresponding fœtus at about two months no sugar could be detected; and he has made numerous other experiments, with similar results, not only on fœtal calves of various ages, but on the human fœtus, and that of the rabbit, goat, sheep, and guinea-pig.

He gives the following results regarding the per-centage of sugar in the liver of the fœtus of various animals:

Human fœtus at six months and a half	0·77 per cent.
Fœtus of calf at from seven to eight months	0·80 „
Fœtus of the cat at the full time	1·27 „

But there is other and even stronger evidence that the sugar is not localized, but produced in the liver. Far from remaining and being hoarded up in that gland, the sugar is undergoing perpetual destruction and renovation, and we can induce its disappearance (by preventing its

re-formation), if we cause an animal to die slowly, as, for instance, by dividing its pneumogastric nerves. Similarly, when the function of the liver is disturbed by severe, and especially by acute, diseases, the formation of sugar is often arrested, and none is found in the liver after death; when, however, death supervenes rapidly—that is to say, when the nutritive faculties have not been suspended for any length of time—sugar may usually be found.

Lastly, not only is the hepatic sugar not dependent on *une alimentation antérieure*, but its amount is in no way connected with the nature of the animal's diet. Two dogs were fed solely on flesh, three on bread and meat, and two on amylaceous or saccharine food, and they were all killed at as nearly as possible the same period of digestion; and the results of the chemical examination of their livers fully bore out this conclusion.

We now come to the question of how the sugar is formed in the liver.

"We have to consider a gland which gives origin to two products—to the sugar which enters the blood, and to the bile which is given off outwardly. What relation exists between these two concomitant phenomena? or are they independent of one another? Can we suppose, for example, that the albuminous matters of the blood on coming in contact with the hepatic cells, break up into two products—a hydrocarbon, which is to form sugar, and a nitrogenous product for the formation of bile? If this were the case, these two products would be formed simultaneously; but the experiments which have been made seem to indicate that the sugar is not formed at the same moment as the bile, and that there is a sort of alternation between these two formations, one of them appearing to be arrested when the other attains its greatest intensity."*

Comparative anatomy confirms the view that these two secretions are independent of one another. Numerous experiments have been made by Bernard on the common grey slug (*Limax flava*), whose liver always contains sugar, and which lives almost exclusively on wood-lice and grubs, and is therefore an animal-feeder. In these animals the order of succession of the digestive phenomena has been carefully watched. After these slugs have been fasting for some time, their stomach and intestines are found to contain a little bile, but no trace of saccharine matter. Shortly after taking food there is a secretion of acid gastric juice, but this mixture contains no trace of sugar. When, however, the dissolved food has passed almost entirely from the stomach into the intestine, a colourless saccharine fluid enters the stomach by the ductus choledochus, which opens near the pyloric extremity. As the intestinal absorption proceeds, the secretion of this saccharine fluid in the liver becomes more abundant, and at length not only fills the stomach, but also the ductus choledochus; and by its backward pressure causes a very distinct and remarkable dilatation of the liver. This general distension of these organs soon diminishes, in consequence of the absorption of the fluid, which seems to be effected solely by the walls of the stomach, scarcely any of it passing into the intestine; and when this secretion has almost disappeared, the ductus choledochus begins to pour forth a fluid which gradually becomes less saccharine and more coloured, till at length, towards the end of the digestive process, pure bile, altogether devoid of

* Leçons, &c., p. 90.

sugar, is effused, such as is found in the stomach of the slug after fasting; and the turgescence of the liver then disappears. Here, then, we have ocular demonstration of the independence of these two functions of the liver. Bernard believes that in the articulata and especially in insects, he has made out the anatomical distinction between that portion of the liver which is to produce bile, and that which is to secrete sugar; but we regard this evidence to be less trustworthy than that yielded by the slugs.

From these observations, Bernard attempts to give a hypothetical explanation of the minute structure of the liver in the vertebrated animals; his view, that there are distinct cells for the secretion of each product, does not, however, appear to be satisfactorily established. Like the other secretions, that of sugar is constantly oscillating between certain limits. Before proceeding to show the causes of these oscillations, he demonstrates the absence of sugar in the different secretions and excretions—in the saliva, the urine, and the bile; and hence he infers that its retention within the organism indicates that it must serve some special purpose; and as, further, it does not exist in the blood of different vessels in anything like the same proportions, it must obviously be in great part destroyed. As it is being constantly formed in the liver, and yet never exceeds a certain limit, at least in the physiological state, it is obvious that there must be a perfect equilibrium between its formation and destruction.

We shall trace the course taken by the sugar from its starting-point—the liver—onwards:—

"Secreted by the hepatic cells, the sugar passes with the blood of the capillaries into the hepatic veins, and from thence into the vena cava ascendens. It is at the point of discharge of the last-named vessel, that the blood is the most strongly saccharine; it then becomes mixed with the blood from the lower parts of the body, and passes up to the right auricle, where the sugar undergoes a new dilution from its admixture with the blood of the vena cava descendens. From the right auricle it passes into the right ventricle, and thence to the lung. In the whole of the route from the liver to the lung, the blood is constantly saccharine, but the amount of sugar varies extremely, and is least at the greatest distance from the liver. In the lung, the sugar, being brought into contact with the air and mixing with the whole mass of the blood, sometimes completely disappears.

"These two organs, then—the liver and the lung—stand in an inverse relation to one another, in so far as the saccharine matter is concerned. In a fasting animal, for example, the blood which arrives at the liver contains no trace of sugar, while that which leaves it is distinctly saccharine. Inversely, the blood which arrives at the lung contains sugar, while that which leaves it contains no traces of this constituent. The sugar, in this physiological state, remains hidden between the liver and the lung, and this is the reason why its existence and formation within the animal body were not earlier discovered. The analysis of blood drawn from superficial veins would fail to detect it under these conditions."*

There are, however, physiological conditions under which sugar may be found in the blood beyond the lungs. During digestion, the liver, in place of merely receiving the returned blood of the mesenteric vessels, &c., additionally receives the whole of the soluble matters absorbed by the capillaries of the portal vein—a quantity twice or thrice as great in some

* Leçons, &c., pp. 105–6.

cases as when the animal is fasting. This organ consequently becomes engorged with blood, and considerably enlarged. The usually slow circulation now becomes singularly active, and the wave of blood which thus enters it probably displaces most of the sugar which had been previously formed, and projects it into the general circulation. Independently of the augmented activity due to the afflux of blood, the liver is also stimulated by the nervous system under the influence of the natural excitation induced by the digestion of food. For these reasons, the activity of the glycogenic function increases with the augmented flow of blood to the liver, and in the course of four or five hours after the commencement of intestinal digestion, the production of sugar in the liver attains its maximum of intensity. Hence—although, as has been previously shown, the nature of the food exerts no influence on the production of this sugar — the period of digestion exercises a very evident influence. Some time, then, after the ingestion of food, and during three or four hours, the production exceeds the destruction of sugar, and there is a temporary excess of this substance in the organism, and that portion which escapes being consumed in the lungs, passes onwards into the arterial system. "At this period of digestion, we find sugar in all the vessels of the body, both arteries and veins; we even find it in the renal arteries, but in too small quantity to pass into the urine." In about six or seven hours after the meal, the excess of sugar in the blood begins to disappear, and the equilibrium between its production and its destruction begins to be restored.

There is, however, one liquid in the animal economy into which, according to Bernard, sugar always passes, even when it only reaches the general circulation in very small quantity,—namely, the cerebro-spinal fluid. He has constantly detected sugar in it (we presume by the reduction of copper only, as he makes no reference to his tests in this instance) in dogs, cats, and rabbits, both when fasting and during digestion—a fact which accords with the observation made several years ago by Magendie, that this fluid is one into which substances introduced into the blood pass with the greatest facility. If, however, food be withheld from an animal beyond a given time, the sugar can no longer be detected. In reference to this subject we ought to observe that M. Bussy,* who has carefully examined the cerebro-spinal fluid which escaped from a man with a fracture at the base of the cranium, and likewise this fluid in the horse and the dog, found that although it reduced the oxide of copper, it could not be made to undergo fermentation; and as other organic substances (leucine and allantoine, for example) possess this reducing power, the reduction-test alone cannot be relied on as affording certain evidence of the presence of sugar. Messrs. Paget and Turner† have recently attempted to determine whether sugar was actually present in these cases, and the latter gentleman examined three separate portions of the cerebro-spinal fluid, obtained by puncturing a spina bifida in a child, several days intervening between the removal of each portion. The three specimens corresponded in giving no indication of grape-sugar,

* Bulletin de l'Acad. de Médecine. Dec. 1852.
† Proceedings of the Royal Society of London, June 15th, 1854, vol. vii. p. 89.

except with Trommer's test—Moore's, Maumené's, and the fermentation-test yielding negative results.

Similar oscillations of the "glycogenic function" to those which we have described, occur in an exaggerated form in diabetic patients; and the preceding observations elucidate the cases of intermittent diabetes described by Rayer, Traube, and others, in which the urine of digestion is saccharine, while no sugar can be detected in the urine at other periods.

The circumstances modifying the secretion of sugar are next considered.

It would be interesting to ascertain, if it were possible, the effect of various changes in the hepatic tissue on the secretion of sugar. In fatty liver, induced artificially in ducks, Bernard found, to his surprise, that the quantity of sugar was increased rather than diminished. Most local alterations of the liver—such as cysts, hydatids, tumours, &c.—appear only to have the effect of diminishing the secreting mass of the liver, for in the immediate neighbourhood of these lesions we find sugar in the ordinary proportions.

Before noticing the influence of different kinds of food upon the production of hepatic sugar, he investigates the effect of entire abstinence from food of any kind on the glycogenic function. Four dogs of the same age, and as nearly as possible the same weight, were selected for this experiment.

"During the first days of abstinence the secretion of sugar goes on to a considerable extent; for in a dog that had fasted thirty-six hours I found 1·255 parts of sugar in 100 of liver; and in another dog that fasted four days, there was 0·93 of sugar in 100 parts. On the following days, the quantity of sugar that is formed diminishes more rapidly till the animal has lost four-tenths of its weight, and is past recovery. I have never found sugar in the tissue of the liver of dogs, rabbits, or guinea-pigs, that died from starvation. . . . . The time necessary for the total stoppage of the production of sugar in the liver under the influence of abstinence, varies with the age and size of the animals, with their class, species, and power of resisting inanition. Amongst the vertebrata, birds most rapidly lose the power of forming sugar in the liver. Thus, in from thirty-six to forty-eight hours sugar ceases to be found in the livers of small birds, such as sparrows. Next to birds come mammals, especially young ones. I have experimented in reference to this point on rats, dogs, cats, and horses. In the rats and in rabbits, from four to eight days sufficed; and in dogs, cats, and horses, from twelve to twenty days sufficed to cause the complete disappearance of sugar from the liver. . . . . Reptiles and fishes differ from warm-blooded animals in resisting for a much longer period the effects of abstinence, and in the slower disappearance of sugar from the liver. Thus, toads, adders, and carp exhibit very evident indications of sugar in the liver after four or six weeks' abstinence. . . . . As the sugar disappears, the respiration, which is intimately connected with its destruction, becomes slower."*

These observations on the effects of abstinence do not, however, apply to hybernating animals during the period of their winter sleep, as has been distinctly shown by Valentin, in his memoir "On the Existence of Sugar in the Liver and other parts of Hybernating Animals," published in volume xiii. of this Review.

The influence of a fatty diet is remarkable. Two dogs were fed on fat

* Leçons, &c., pp. 129–131.

bacon from which all the lean had been removed, and on hog's lard, for three and for eight days respectively, and the singular result was obtained, that under the influence of this food there was a positive diminution of the sugar in the liver, such as would have occurred if the animals had been rigidly fasting, the sugar in these cases amounting to 0·88 and 0·57 per cent. Bernard explains this result in the following manner: the fundamental principles of all foods are reducible to three classes—the albuminous, the farinaceous or saccharine, and the fatty kinds of food; of these, the last alone do not pass through the liver, but are absorbed directly by the lacteals, and hence they do not affect the portal blood.

In order to investigate the effect of a nitrogenous diet, two dogs were fed solely on gelatine and gelatinous matters for some days. Under the influence of this food the quantities of sugar found in the liver were as nearly as possible normal (being 1·33 and 1·65 per cent.), although one of the dogs had been kept without any kind of nourishment for four days before the experiment commenced. "Hence," says Bernard, "it is the nitrogenous element which serves to form the sugar, and chemistry confirms that which physiology indicates; for Lehmann has proved that the portal blood in traversing the liver loses a certain quantity of its nitrogenous principles, and that the fibrin is distinctly diminished." As in the case of animals fed solely on nitrogenous food, neither the intestine nor the portal blood contains sugar, we can arrive at no other conclusion than that the sugar which is found under these conditions is the result of the action of the liver on the albuminous principles which have entered the portal blood.

The effect of a farinaceous diet was then observed upon two dogs—a subject of special interest, "in consequence of the care taken by all physicians to exclude every trace of starch and sugar from the diet of their diabetic patients." The first dog, after being starved for four days, was fed for six days on starch and water; the second dog, without any preliminary starving, was fed for three days upon a mixture of mashed potatoes, starch, and sugar, with a little water. In the liver of the first dog there was 1·25 per cent., and in that of the second 1·88 per cent. of sugar—numbers which do not materially differ from those which are yielded during a gelatinous or mixed diet. In a physiological state, then, the ingestion of amylaceous or saccharine matter does not augment the quantity of sugar in the liver, and consequently in the animal economy generally; although in cases of diabetes the use of these substances commonly causes a great and immediate augmentation of the sugar in the urine.

The influences of various diseases, of temperature, age, &c., on the glycogenic function of the liver, are considered in a subsequent lecture. It appears, from Bernard's researches, that severe diseases, whether acute or chronic, but especially if acute febrile symptoms are present, very rapidly put a stop to the production of sugar in the liver; and this is the reason why sugar is so often sought for fruitlessly in the livers of hospital patients. When a diabetic patient is seized with another disease, the same thing holds good. The sugar is no longer secreted, or, at all events, the secretion is much diminished, and the urine is no longer saccharine. When, however, the secondary disease abates, the sugar re-appears.

Bernard notices a singular case of this kind that fell under his own observation. A diabetic woman had a chronic affection of the bowels, which occasionally assumed an acute form, when she suffered from colic and diarrhœa; whenever the last-named symptoms appeared, the urine ceased for the time to be saccharine. In this patient he saw the sugar vanish and re-appear five or six times under these circumstances. In the last stage of diabetes, when phthisical symptoms have supervened, and the digestive functions are much disturbed, the sugar often disappears from the urine; and this may be taken as a sign that death will speedily occur.

The influence of external temperature on the hepatic functions was determined experimentally on guinea-pigs and rabbits. On exposing these animals to great cold, so as to reduce the temperature from the normal standard of about 100° to 68° Fahr., the hepatic sugar entirely disappears in the course of two hours; while on placing them in a surrounding medium of a temperature rather above the bodily heat—as, for instance, in a stove at 113° Fahr.—there is an exaltation of the functions of the liver, which, however, is more marked in relation to the secretion of bile than to that of sugar. But if the temperature be raised to 120° or 130°, an opposite effect is produced: the sugar disappears, and the animal dies in from one to two hours, without a trace of saccharine matter in its liver.

Neither age, sex, pregnancy, nor lactation, seems to exert any special influence on the formation of sugar in the liver.

We now proceed to consider the uses and final destiny of the hepatic sugar in the organism.

As farinaceous and saccharine food does not increase the amount of the sugar that is found in the tissue of the liver, it is in the highest degree probable that the sugar derived from the food plays an entirely different part in the animal economy from the hepatic sugar. Bernard finds that, if we take two dogs, and feed one exclusively on flesh and the other exclusively on amylaceous matters for some days, and then kill them, the watery decoction of the liver will be perfectly limpid in the first case, while in the second it will be "turbid, opalescent, and of a milky appearance." The two fluids will be found equally to abound in sugar, but the latter also holds in suspension an emulsive matter—a mixture apparently of a fatty and of a protein body; and from this he infers that the sugar yielded by the food does not pass in the form of sugar into the blood, but that it is converted by the liver into fat. We have not space to notice the various arguments or the experiments by which he supports this view; and we will merely remark that Lehmann, who has published a very elaborate criticism of Bernard's Leçons, in vol. lxxxviii. of Schmidt's 'Jahrbücher,' regards the arguments as inconclusive, and the experiments (which are in direct opposition to those of von Becker, who worked under Lehmann's direct superintendence) as fallacious. The question as to what becomes of the alimentary sugar is still involved in considerable obscurity. We turned to Dr. Bryden's Harveian Prize Essay, to see if he had thrown any new light upon it, but we do not find that he has made any experiments on the subject; and his conclusion, "that it is as lactic acid, or more probably as lactates--the base being derived from the

biliary and other secretions—that the variety of sugar which we have been considering enters the circulation," is, we think, by no means satisfactorily established. It is, indeed, in a great measure based on an isolated observation in page 278 of the third volume of Lehmann's 'Physiological Chemistry,' which can hardly be taken as a fair expression of that eminent chemist's views on the subject; for in his latest work we find the following sentence, which tells most decisively against Dr. Bryden's theory:—"From the augmentation in the amount of sugar in the blood after the ingestion of sugar, it follows that the greatest part of the glucose is absorbed in an unchanged state; a fraction of the sugar is, however, always converted into acids."* Nor does Dr. Pavy at all remove our difficulties on this point: he merely tells us that, from experiments performed in M. Bernard's laboratory, it appears that the liver—

"Exerts some modifying influence on the sugar which is traversing its capillaries, and which has been absorbed from the food, by which it is transformed from vegetable into animal sugar or glucose, and thus rendered more apt for being subsequently destroyed by the processes of animal life."†

We must confess that we do not clearly comprehend the differences between animal and vegetable sugar to which Dr. Pavy refers; and we have chiefly noticed these discrepant opinions with the view of directing further inquiry to the subject.

We now proceed to give a sketch of Bernard's views regarding the destruction of the hepatic sugar in the blood. The theory of the oxidation or combustion of the sugar in the lungs is first discussed. If the sugar were actually destroyed in the lungs by coming in contact with the oxygen of the air, any cause that disturbed the due performance of the respiratory functions—as a more or less perfect occlusion of the air-passages, or the inhalation of air mixed with certain vapours, as those of ether or chloroform, or the respiration of air poor in oxygen—would prevent the destruction of the sugar, and allow it to pass into the general circulation, and consequently into the urine. M. Reynoso found that, under the influence of ether, the urine became temporarily saccharine, and at once referred the result to the deficient oxidation of the sugar in the lungs. The fact is correct enough, but Bernard shows, by the following singular experiment, that it admits of an altogether different explanation: If a dog or a rabbit be taken for experiment just after the digestion of a meal has been fully accomplished, and if we draw blood from its jugular vein, this blood will contain no appreciable traces of sugar; if, however, we compress its abdomen so as to exercise a certain pressure on the liver, or if we excite violent contractions of the abdominal muscles and of the diaphragm by carefully closing the nostrils for some minutes, and then draw blood from the jugular vein, we shall find that the fluid is saccharine. The compression of the liver causes a sudden and excessive effusion of sugar into the blood, and the passage of a portion of it into the general circulation and the urine. Bernard believes that, in Reynoso's experi-

* Handbuch der Physiologischen Chemie, p. 265. Leipzig, 1854.

† Guy's Hospital Reports, third series, vol. i. p. 20. As in one or two cases Dr. Pavy makes statements regarding Bernard's views that are not strictly in accordance with those expressed in the Leçons, we think it right to mention that the article from which we quote was written previously to the publication of Bernard's volume.

ments, the presence of the sugar was due to the muscular efforts and contractions of the animal, although ether and chloroform exert a special action in this respect, to which we shall presently allude. At first he seems to have inclined strongly towards the oxidation theory, and this view was confirmed by his finding a saccharine fluid in the urinary bladder of the fœtus of the calf, at the fourth or fifth month; but on examining the urine at different stages of fœtal existence, he discovered other facts that could not be explained by this theory. He found that in the calf the glycogenic function of the liver does not commence before the fourth month of fœtal life; but contrary to his expectation, he also found the urine of the fœtus, at a very early stage, to be highly saccharine when the liver does not yield a trace of sugar. On the other hand, the tissue of the liver contains an increasing quantity in proportion as the period of gestation approaches, and hence we might naturally infer that the urine would become more saccharine as the period of birth drew near. But this is not the case: the urine of the fœtal calf, at the sixth or seventh month, ceases to contain sugar,* although it is then secreted in the organism, and is found in large quantity in the liver. Bernard next examined the direct influence of oxygen on the destruction of the sugar in blood obtained from the hepatic veins, but to his surprise he found that the sugar was not decomposed, even after five or six hours, in blood saturated with oxygen; while it was decomposed with considerable rapidity by nitrogen and hydrogen, and in still shorter time by arseniuretted hydrogen. These experiments with the gases, however, are in reality of less weight than Bernard supposes, because, as Lehmann has observed, many of them have a tendency to promote the decomposition of the blood.

The next theory, or rather hypothesis, is, that the destruction of the sugar in the organism is due to the combustion of the sugar with the co-operation of an alkali. Bernard makes various experiments, similar to those of Lehmann and v. Becker,† and arrives at the same conclusion—namely, that sugar, when injected into the jugular vein, in association with potash or its carbonate, is not destroyed in greater quantity than under ordinary circumstances.‡

There are only two ways in which organic matters can be destroyed, either by a process of oxidation or of fermentation. As oxidation fails to account for the phenomena in question, we must fall back on fermentation, which, as we know, is the main agent in a host of transformations in both the vegetable and the animal kingdoms. The conditions necessary for alcoholic fermentation are absent in the organism; and if they are artificially supplied—if, for instance, a mixture of sugar and yeast be injected into the veins of an animal—death is the certain result. The destruction of the sugar in this manner is consequently impossible; but our author believes that under the influence of the extreme division which it undergoes in the blood, it may be converted into lactic acid by

* These observations accord with those recently published by Dr. W. D. Moore, who failed to detect sugar in the urine of the human fœtus at the full period.

† See Lehmann's Physiological Chemistry, vol. iii. p. 234.

‡ Still further evidence on this point may be found in a memoir by Poggiale, On the Action of Alkalies on Sugar, in the Comptes Rendus, tom. xlii. p. 198. Paris, 1856.

a simple molecular change, in which the oxygen plays only a secondary part. The researches of Dr. Pavy throw considerable light upon this subject, and strongly corroborate Bernard's view.

"In experiments which the author (Dr. Pavy) has now several times repeated, he injected blood removed from the right side of the heart of an animal—and therefore normally containing sugar—through the capillaries of the artificially inflated lungs of another; and found that as long as the blood retains its fibrin, there is as much destruction of its sugar as would take place in the living animal; but that where the fibrin has been separated from the serum and corpuscles, the sugar ceases to be influenced by the presence of oxygen, or ceases to disappear during the process of artificial respiration. It would hence appear that something besides mere contact with oxygen is requisite for the destruction of sugar. But in other experiments he has found that oxygen is nevertheless a necessary agent concerned in the process of transformation observed during the arterialization of the blood that has not undergone spontaneous coagulation. It would therefore seem, in fact, that oxygen acts secondarily on the sugar, through the medium of the fibrinous constituent of the blood; that it exerts some changes upon this azotised principle, which are capable of inducing the metamorphosis of sugar. . . . If the molecular changes occurring during the decomposition of an azotised substance be capable of converting sugar ($C_{12}H_{12}O_{12}$) into lactic acid ($C_6H_6O_6$), why should not the molecular changes occurring during the building-up or elaboration of this same nitrogenized compound effect the same? Indeed, we have seen that the process of destruction is carried on to a certain extent in the systemic capillaries, and more especially in those of the chylopoietic viscera, where the molecular changes of nutrition are also correspondingly carried on with greater activity than elsewhere. So that analogy and experiment would tend to show that the physiological destruction of sugar is owing to a process similar to fermentation, induced by the molecular changes occurring in the nitrogenized constituents of the animal during life. And in accordance with this, we find lactic acid present in the system, and largely separated from arterial blood by the muscular tissue and the secerning follicles of the stomach. As regards the lactic-acid fermentation, it is well known that the presence of an alkali favours, while that of an acid retards, the process. In two experiments on animals, the author injected carbonate of soda and phosphoric acid into the circulating current, and observed, in the case of the latter, that sugar immediately accumulated in the blood."*

In proceeding to notice the uses of sugar—which he does in his twelfth lecture—Bernard observes that it is difficult at first sight to perceive what part this substance plays in the organism. As it is constantly produced in the liver from a certain epoch of intra-uterine life to the death of the animal, we cannot doubt but that it must have important functions to fulfil. Some physiologists maintain, that by its destruction it developes the heat necessary to support the animal temperature; but this is a mere supposition, not only unsupported by evidence, but in direct opposition to the fact observed by Bernard, that the greatest heat is produced during the formation of sugar in the liver, and not during its destruction, the blood which leaves that organ by the hepatic veins being found to exhibit a higher temperature than the blood in any other part of the body.† The actual uses of sugar in the animal economy are so fully described by Lehmann, in the third volume of his 'Physiological Chemistry,' pp. 216–221, that it is unnecessary for us to advert to them here. Most of

* Proceedings of the Royal Society, vol. vii. pp. 373–4.

† Bernard found that in a dog the temperature of the portal blood entering the liver was 102°·92 Fahr., and that of the hepatic veins 103°·61, while the aortic blood only raised the thermometer to 101°·66. (See Leçons, &c., p. 200.)

these uses are altogether ignored by Bernard, who has discovered that it has a new use, *d'une bien plus grande importance.* He believes that he has proved the presence of sugar to be as necessary for the development of animal as of vegetable tissues; in short, that the action of sugar is essential in the development of organic cells generally. As we do not think that he has succeeded in establishing his case, we shall not describe the experiments which led him to entertain this idea. We shall, however, take this opportunity of noticing more fully the observations which he has made in reference to the occurrence of sugar in the organism during fœtal life (to which allusion has been made in p. 30), inasmuch as it was the theory regarding the importance of sugar in relation to the development of the tissues, that led him to inquire whether sugar did not play an important part, and was not to be detected chemically, in the animal body when its evolution was proceeding with the greatest energy—namely, during intra-uterine existence. His observations on the occurrence of sugar in the fœtal tissues are scattered through various parts of his volume.* The following are his chief results in connexion with this subject:

1. If fœtal lungs or muscular tissue (either voluntary or involuntary) be placed in water at a temperature of 60° or a little higher, a very considerable quantity of lactic acid is developed; whereas, if corresponding adult tissues are similarly treated, ammoniacal products are formed, and the water presents an alkaline reaction. By taking means to arrest the lactic fermentation, undoubted evidence of the presence of glucose in those tissues may be obtained both by Trommer's test and by fermentation. We have not as yet been able to isolate the substance from the lungs or muscles which gives rise to the sugar; but we know that it exists in these tissues in a state insoluble in water, alcohol, or ether; for not only do the above-named tissues yield no sugar to those fluids, but after soaking in these menstrua they still yield sugar and lactic acid. When the tissues are once fairly developed, generally about the fifth month of intra-uterine life (in the fœtal calf, whose period is the same as that of the human fœtus), this property diminishes, and at about the eighth or ninth month, when the muscular elements are definitely formed, the production of sugar in these tissues entirely ceases.

2. While sugar can be discovered in the lungs and muscles, it cannot be detected in the glandular or nervous systems, in the skin, or in the bones; and (contrary to what might have been expected) the liver, which, when the functions are duly localized, becomes the great glycogenic organ, is, during the earlier period of embryonic life, as free from sugar as the other glandular structures. In the fœtal calf, it is not till about the fourth or fifth month that sugar in small quantity begins to appear in the liver, but from that period the quantity of hepatic sugar increases with the age of the fœtus.†

3. Until about the middle of intra-uterine life, the saccharine matter which is formed in the pulmonary and muscular tissues is not converted into lactic acid (or otherwise destroyed) almost immediately after its

* See Lecture XI. pp. 230–232; Lecture XII. pp. 248–254; Lecture XX. pp. 380–389; and Lecture XXI. pp. 393–403.

† See Leçons, &c., p. 82.

formation, as occurs in adult life, but enters into the circulation, and thus makes its way into the urine; and the urine accumulating in great quantity, fills not only the bladder, but the allantois, which communicates with it, and thus the allantoic fluid also becomes saccharine. The liquor amnii also contains sugar (but in much smaller quantities), which must either have got there by endosmosis, or must be due to a small quantity of urine having been expelled from the bladder through the urethra.

4. Independently of its use (according to Bernard's view) in connexion with the development of the tissues, he shows, by ingenious experiments, that its presence in the blood prevents the infiltration of that fluid into the tissues, and promotes the circulation generally. In the liquor amnii the sugar undergoes a kind of viscous fermentation, which gives to that fluid its well-known glutinous character; so that, as Bernard observes—

> "The advocates of final causes might perceive here a secondary use of sugar during intra-uterine life; having first prevented the imbibition and infiltration of the young and delicate tissues by the liquor amnii, it becomes converted before the period of parturition into a viscid substance, capable of lubricating the passages and facilitating the escape of the fœtus."

The mechanism of the formation of sugar in the liver next claims our attention. That a secretion may be produced, it is an established law that two conditions are imperative—namely, 1, the blood; and 2, a glandular organ to which the blood goes. It is a doctrine generally accepted by the physiologists of the present day, that the glandular organ furnishes nothing to the secretion, but that its tissue (or, at all events, certain of its cells) exerts a catalytic action on the elements of the blood as it traverses the organ. In accordance with this view, Lehmann has afforded us a very satisfactory explanation of the origin of the sugar in the liver. On comparing the composition of the blood of the portal and the hepatic veins, he found that the saccharine blood of the hepatic veins contains less fibrin and less hæmatin than the non-saccharine blood which enters the liver by the portal vein. He then proved, by a very ingenious chemical process, that pure crystallized hæmatin might be resolved into glucose conjugated with a nitrogenous substance; and from this he infers that the liver most probably disintegrates this, and perhaps some other constituents of the portal blood, in a somewhat similar manner, into glucose and a nitrogenous substance which enters into the composition of the bile. Since the publication of his Leçons, Bernard has, however, been led to give up Lehmann's explanation, and has been driven to the belief, from certain experiments which he has recently made, that *it is not in the blood, but in the hepatic tissue itself, that we must search for the substance which precedes and directly gives origin to the sugar.* The subject is one of such importance that we give the main points of the leading experiment.

A dog that had been fed exclusively on flesh for several days was killed seven hours after a meal; the liver was carefully removed, without injury or the slightest delay, and a current of cold water was injected with considerable pressure into the portal vein before the blood had had time to coagulate, and continued for forty minutes. The liver gradually assumed an exsanguine appearance, and the water which jetted out of the hepatic veins, from being bloody and saccharine, became colourless, and entirely

free both from sugar and from albuminous matters. The liver was afterwards submitted to the external action of a current of water; and on boiling a small portion, it was found that its decoction did not give the slightest trace of sugar. On exposing the organ for twenty-four hours to the action of the atmosphere, it was found that this structure, which on the previous day was completely free from sugar, now contained it in abundance.

This experiment clearly proves (says Bernard) that in the healthy liver there are two substances,—(1), sugar which is very soluble in water, and which is carried off during the injection into the portal vein; and (2), a substance so slightly soluble in water, that it remains fixed in the hepatic tissue after the blood and sugar have been removed by the prolonged washing. It is this latter substance which, in a liver left to itself for a sufficient time, becomes converted into sugar by a kind of fermentation.

This formation of sugar generally terminates in about twenty-four hours, and if after that time we again inject and wash the liver, so as to remove all the newly-formed sugar, we seldom find that any more is produced, the substance yielding it being doubtless exhausted; the formation is, however, effected more rapidly when we increase the surfaces which are in contact with the air, by cutting it in slices, and moistening them with water.

We shall devote the short space that still remains to us to the consideration of "artificial diabetes."

We find, as might be expected, minute details regarding the mode in which Bernard performs his celebrated experiment of inducing artificial diabetes, by pricking a certain point of the medulla oblongata either of a herbivorous or a carnivorous animal; but until we read these lectures, we were not aware that he had extended this experiment in the manner described in the following paragraph:—

"When we prick the mesial line of the floor of the fourth ventricle in the exact centre of the space between the origins of the auditory and pneumogastric nerves, we at the same time produce an exaggeration of the hepatic [saccharine] and of the renal secretions; if the puncture be effected a little higher, we very often only produce an augmentation in the quantity of the urine, which then frequently becomes charged with albuminous matters, while if the puncture be below the indicated point, the discharge of sugar alone is observed, and the urine remains turbid and scanty. Hence it appears that we may distinguish two points, of which the inferior corresponds to the secretion of the liver, and the superior to that of the kidneys. As, however, these two points are very near to one another, it often happens that if the instrument enters obliquely, they are simultaneously wounded, and the animal's urine not only becomes superabundant, but at the same time saccharine."*

The urine becomes saccharine in from one to two hours after the operation, but seldom continues so for more than a day.

It is gratifying, and, we must confess, somewhat surprising, to find that M. Bernard's animals do not seem to suffer much pain or inconvenience from his scientific investigations. After performing the experiment which has just been described, he observes that the rabbit merely "*semble être un peu étonné sur le moment, mais il remettra assez rapidement.*"

* Leçons, &c., pp. 339-340.

As we have seen it implied that this singular discovery—that by pricking a certain point of the nervous system we could render an animal diabetic—was due to a mere happy chance, we shall give in a very few words Bernard's account of the manner in which he was led to it. Assuming it as a recognised fact that all secretory organs are influenced by the nervous system, which can either augment or depress their secreting functions; and bearing in mind Magendie's experiment, in which, by exciting the lachrymal branch of the fifth pair, he caused an abundant flow of tears, and that they ceased to flow when the nerve was divided; Bernard was led to investigate whether a somewhat corresponding experiment could not be performed on the liver. Indeed, half the experiment was ready prepared for him, for he had previously ascertained that the secretion of sugar in the liver is stopped when the pneumogastric nerves are divided, and it only remained to try the inverse case, and to see whether irritation or stimulation of the nerves would cause an increased secretion of sugar. With this view he galvanized the nerves, but without obtaining the expected result. He then recollected that in experimenting upon a totally different subject—namely, upon the functions of the fifth pair of nerves—he had observed that when, instead of dividing them within the cranium, as he intended, he only pricked the nervous centres at the origin of the nerve, the secretions (tears and saliva), which would have been stopped if the operation had been properly performed, were actually increased to a considerable extent. This led him to attempt to prick the origin of the pneumogastric, and to observe if an analogous effect would be produced to that which he had seen manifested by the secretions which are under the influence of the fifth pair. His very first attempt was successful, and in the course of an hour, the rabbit on which he operated became diabetic, both the blood and the urine being charged with sugar.

Bernard's original theory, that the secretion of sugar was under the direct influence of the pneumogastric nerve, was, however, erroneous, as he shortly discovered; for he found that if before irritating the floor of the fourth ventricle he first divided the pneumogastric, sugar still appeared in the urine. He now believes that the nervous influence on the liver is transmitted by reflex action through the ganglia of the sympathetic. After laying it down as a law that a ganglionic apparatus pertaining to the great sympathetic always exists between the organ which receives the reflex action and the nervous centres which propagate it, he maintains, in relation to the glycogenic functions of the liver, that the starting-point of the irritation is the lung, which is always receiving on its surface the impression of the air; this impression or sensation is perceived by the extremities of the pneumogastric nerves, which are distributed over the lungs, and is thus conveyed to the medulla oblongata, from whence it is propagated by the spinal cord and by filaments of the great sympathetic nerve to the liver. We regret that we cannot find room for a sketch of the anatomical details, or the various ingenious experiments by which he arrived at the above conclusion.

Other means of producing artificial diabetes are subsequently described. It appears that any agents or conditions that cause a suspension of the functions of animal life, while the purely nutritive or organic functions

remain intact, induce the diabetic state. In this way the celebrated Indian poison, *Curare*, and apoplexy induced by a severe blow on the skull, have been shown by Bernard to produce saccharine urine. Moreover, local irritation of the liver may augment the glycogenic functions. M. Harlay (who is probably better known in this country as Dr. Harley) injected irritating substances—such as a dilute solution of ammonia or ether—into a branch of the portal vein, and after some time found sugar in the urine. Hence it is not impossible that abnormal matters may be sometimes absorbed from the intestines by the mesenteric veins, and produce a similar effect; and we may thus probably explain the fact that Leconte always found sugar in the urine of dogs slowly poisoned with small doses of nitrate of uranium. To somewhat similar causes we may also refer certain cases of diabetes produced by contusions in the region of the liver—such, for instance, as one mentioned by Bernard, in which a man became diabetic from receiving a kick in the right hypochondrium from a horse. The sugar disappeared when the patient recovered from his contusion, but he continued to pass an excess of urine.

We regret that our limited space prevents us from noticing, even in the briefest manner, Bernard's experiments upon the influence of the spinal cord on the formation of sugar, or his lectures on the application of his physiological discoveries to the pathology of diabetes; and we cannot conclude this short sketch of the 'Leçons de Physiologie Expérimentale' without assuring our readers that in the preceding pages we have merely given them an average sample of the physiological riches with which this volume abounds.

Dr. Bryden's prize essay contains an excellent *résumé* of nearly all that is known on the sugar question, and there are several points which he has illustrated by original observations and analyses. He is, as far as we know, the only British observer who has succeeded in confirming Reynoso's statement. that the internal use of arsenic and quinine gives rise to saccharine urine. A small volume "On the Physiological Relations of Sugar" would be a welcome addition to our medical literature; and we hope soon to see Dr. Bryden's essay (or, at all events, the more important chapters of it) in print.

Review IV.

The Obstetric Memoirs and Contributions of James Y. Simpson, M.D., F.R.S.E., Professor of Midwifery in the University of Edinburgh, &c. Edited by W. O. Priestley, M.D. Edinburgh; and Horatio R. Storrer, M.D., Boston, U.S. Vol. II.—*Edinburgh*, 1856. pp. 819.

The present volume of Dr. Simpson's obstetric writings, although nearly equal in point of magnitude to its predecessor, contains fewer materials for either analysis or criticism. Of the entire work, upwards of a third is occupied with papers on the subject of anæsthesia, many of which are now possessed of historical interest only; whilst the remainder consists for the most part of a series of disquisitions upon the physiology and pathology of the products of conception, which, although replete with

interesting facts and ingenious speculations, are rather adapted for specific reference than for critical analysis. Our notice of the volume will therefore be restricted to comparatively small portions of it only;—to such papers as are of a practical rather than of a speculative character;—and these will be found more especially in those sections of the book which treat of the pathology of the puerperal state, and that of infancy and childhood.

Two papers on the subject of puerperal fever first claim our consideration, of which the first is devoted to a consideration of the analogy which subsists between it and "surgical fever;" and the second, to its communicability and mode of propagation. It is believed by the author that puerperal and surgical fever are assimilated to each other in the following respects:—1. In the anatomical conditions and constitutional peculiarities of those who are the subjects of them. 2. In the pathological nature of the attendant fever. 3. In the morbid lesions respectively left by either disease; and 4. In the symptoms which accompany each affection. We subjoin the principal facts which are alleged in support of each of these analogies.

I. The anatomical conditions of the puerperal patient after delivery, and of the surgical patient after an operation, are represented as being in many respects the same. In both there is a wound or solution of continuity; in the latter case, on some external part of the body, in the former, on the internal surface of the uterus, caused by the separation of the placenta and the exfoliation of the decidua. On the surface of both of these wounds numerous arteries and veins open; both are repaired by the exudation of organizable lymph, and the reparation of either is liable to be complicated with various constitutional states of the same kind. Both may be followed by symptoms of shock or collapse; both have generally a subsequent limited degree of febrile action; and in each case the wound is liable to deviate from the standard mode of reparation, for their secretions may alter morbidly, or they may become the seat of an excess of inflammation or ulceration, or of phlebitic suppuration and its consequences. In both cases air occasionally enters by the mouths of the veins which open upon the free surface of either wound; from both, dangerous hæmorrhage, both primary and secondary, is liable to occur. Both are occasionally, though very rarely, followed by delirium, tetanus, and other nervous complications; and in like manner, but much more frequently, they are apt to be followed by that form of combined febrile and inflammatory action which we term surgical fever in the surgical patient, and puerperal fever in the puerperal patient In short, it is averred that the two species of wounds are subject to the same local pathological deviations, and liable to be attended with the same pathological constitutional effects and complications. II. The analogy in the pathological nature of puerperal and surgical fever is chiefly based upon the doctrine that the real source and cause of both is to be found in a toxæmia or morbid state of the circulating fluid. After commenting upon the insufficiency of the theories which were formerly held regarding the nature of puerperal fever, one of which viewed it as an idiopathic fever *sui generis*, the other as essentially a local inflammation, upon which the fever was dependent, the author refers to the experi-

ments made by Gaspard, Cruveilhier, Castlenau, and others, upon the lower animals, as showing that by the direct injection of pus and other morbid secretions into their veins, a train of symptoms during life, and a series of lesions observable after death, may be produced, having a very strong analogy to those of puerperal fever. In harmony with this doctrine, he then points out various facts which show that both in the puerperal and surgical patient such conditions exist as facilitate the infection of the general circulation—such as by the absorption of pus and other morbid matters from the uterine and surgical wound through the orifices of open veins, by the inoculation of morbid and contagious matters through the abraded vaginal surface, and by the supervention of any accidental inflammation. Accepting, then, this theory upon these grounds, the author maintains that it affords the best explanation of the relations which subsist between the general febrile action and the internal inflammations which are respectively met with in each case. III. In support of the analogy in regard to the internal pathological lesions left by puerperal and surgical fever, the author gives various tables drawn up by Chevers, Dugès, and Tonnellé, and deduces from them the following conclusions:—1. That both diseases generally leave upon the dead body ample evidence of the occurrence before death of acute and often extensive internal inflammatory action. 2. That the internal inflammatory lesions are seldom limited in the same case to one organ or texture only, but two or more different viscera or surfaces are usually observed to have been either the simultaneous or successive seats of inflammatory action; and the different parts thus attacked are sometimes very distinct and distant from each other. 3. The internal viscera or textures which are the first and principal seats of inflammation are often far removed from the original wound or lesion, particularly in those cases in which the wound or lesion is in the head or extremities; whilst, however, in the case of the obstetric patient, various causes appear to localize inflammatory action upon the uterus, uterine appendages, and peritoneum in puerperal fever, and more especially from their being the immediate seat of injury and lesion in the act of parturition. IV. Lastly, in speaking of the analogy in the symptoms of puerperal and surgical fever, the author remarks that there is almost no disease which varies more than puerperal fever does in different cases, and that the same variability holds good in regard to surgical fever. When, however, the disease is fully marked, the symptoms, he observes, are sufficiently striking and similar in each—the more marked consisting of rigors, a pulse varying in strength, but always of great frequency, an altered and frequently darker or almost icteric hue of surface; the skin sometimes hot and dry, sometimes bathed with perspiration; local pains and functional derangements, anxiety and general prostration, laboured or hurried respiration, and often at last rapid sinking, with or without delirium.

Such are the chief grounds which are alleged in support of the analogy that subsists between puerperal and surgical fever; and admitting the full force of the considerations upon which it is founded, and the ability with which the question has been argued by our author, we must yet be permitted to express our dissent from some parts of the argument, and to state the grounds upon which it is founded.

With regard, then, to the analogy which exists between an amputated stump and the interior of the uterus after childbirth, we may observe that this had been long ago insisted upon by Cruveilhier, upon grounds very similar to those now alleged by our author, and yet the parallel has very generally been felt to have been overdrawn. What analogy, indeed, except the most distant, can possibly exist between the physiological process of childbirth and the proceeding by which an important limb is arbitrarily severed from the body? In the former case there is a gradual preparation for the changes which are about to take place; whilst in the latter there is none. The uterine wound after parturition is limited to that caused by the separation of the placenta and decidua. It is, therefore, essentially superficial, and confined to the mucous membrane of the uterus; whereas that caused by amputation involves a division of the muscular, cutaneous, vascular, and other structures of the limb; and whilst the vessels torn across by the separation of the placenta are extremely friable, elastic, and retractile, as we have particularly noticed in separating the placenta of the bitch; those divided by amputation are far otherwise. Now these circumstances, no less than the contractions of the uterus after childbirth—which, by diminishing the size of the uterine wound, and maintaining a closed state of the torn uterine vessels, must very much lessen the liability to hæmorrhage and other dangers which are so apt to follow upon amputations, and proportionately diminish the analogy which exists between the two conditions.

Nor is it evident that the advancement of puerperal and surgical pathology can be best secured by assimilating, under the generic terms of puerperal and surgical fever, all the varied forms of febrile and inflammatory disease which are so liable to occur respectively after childbirth and operations. Is there, indeed, any special form of fever consequent upon either, so constant and precise in its nature, as to justify these particular appellations? Or is it not rather the case that, in connexion with, or as a consequence of, both, many different forms of febrile and inflammatory disease are liable to supervene, the precise nature of which will vary under different circumstances and in different cases? Now we apprehend that these questions must be answered in the affirmative of the latter; for if we look alone to the statistical tables quoted by Dr. Simpson, we find a series of lesions given, incidental to childbirth and operations, so numerous and dissimilar, that it is impossible to group them together under one common appellation without confounding all rules of nosological classification. Peritonitis, enteritis, pneumonia, pleuritis, bronchitis, laryngitis, diphtheritis, pericarditis, arteritis, phlebitis, meningitis, cerebritis, cystitis, &c., represent in one table the inflammatory lesions which are sometimes consequent upon operations; and an equally formidable list in another represents those which are liable to occur after childbirth. We repeat, that it is impossible to assimilate or group together all these several maladies under one common designation, without an entire disregard of all nosological distinction.

But, admitting them to be nosologically distinct and different, the question arises, are they nevertheless identified in a common origin? or, in other words, can we admit the doctrine contended for by our author,

that all forms of puerperal fever and inflammation, as also all forms of fever and inflammation supervening upon operations, have a common origin in a morbid or vitiated condition of the blood? Now, speaking rather of puerperal fever, which this paper is more particularly intended to elucidate, and reviewing the question in its several bearings, we feel bound to assert that this doctrine is altogether untenable; that the fevers consequent upon childbirth are of a very varied and dissimilar character, some having their origin as certainly in lesions of the solids, as others have their origin in a vitiation of the fluids of the body.

It is not, however, necessary, nor would it be consistent with the scope of this article, to discuss at any length the abstract doctrine of fever; nor do we propose to enter upon it farther than may be sufficient to indicate very generally the grounds upon which we are led to dissent from the doctrine propounded by our author, that the cause of puerperal fever is to be sought for, in all cases, in a morbid condition of the blood. Believing that fever is essentially a disease of the nervous system, we are led to look for its origin, not so much in one as in many modes of causation, and to conclude that its type and character will vary with the nature of the various causes by which it may be induced. Hence the origin of new forms and types of fever in different seasons and at different epochs—the specific differences in fevers arising from different specific causes—the difference observable in the puerperal fever of different epidemics—and the discrepancy in the writings of those who have observed it in different seasons or under different circumstances. At one time the inflammatory, at another the febrile, type prevails; in one epidemic the sthenic, in another the asthenic; but to argue hence that all puerperal fevers are and must be either idiopathic or symptomatic, sthenic or asthenic, would be very greatly to exceed the legitimate bounds of observation and induction; and admitting fully the blood origin of puerperal fever in many cases, we are yet constrained to believe that it may and does frequently arise independently of any primary vitiation of the blood.

Whatever the causes or the varieties in the type of puerperal fever, it is in the character of the attendant lesions, more perhaps than in anything else, that its chief peculiarity consists: in the presence of fever complicated with inflammatory lesions of the pelvic and abdominal viscera.—"The most fatal disease to which lying-in women are subject," observes Dr. Gooch, "is known under the names of puerperal or childbed fever, puerperal peritonitis. Its essential symptoms are, pain and tenderness over the abdomen, with a rapid pulse. It begins a few days after delivery, with pain of the abdomen, shivering, succeeded by heat, and a quick pulse. As the disease advances, the milk becomes suppressed, the belly tumid, and the breath short; when it terminates fatally, it does so commonly about the fifth day, but often in less than half that time. On opening the abdomen, the morbid appearances are not uniform; but the most common and remarkable are, a copious effusion of lymph and serum on the surface and in the cavity of the peritoneum. Thus it is a fever essentially complicated with an affection of the peritoneum. A better name than puerperal fever, or puerperal peritonitis would be that which I have placed at the head of this paper—peritoneal fever; for it would express the fact, that an affection of the peritoneum is an essential

accompaniment of the disease, without defining what that affection is, because it is not uniform."* Dr. Lowder, after extensive reading and observation, found that the pathognomonic symptoms of puerperal fever were very few, and reducible to the following—fever, intense pain of the head, and intense pain of the abdomen; so that we are justified in asserting that, however the disease may differ in different cases, whether it be sthenic or asthenic, irritative, inflammatory, or typhoid, that it is in the disposition to such abdominal and pelvic lesions that its chief peculiarity as a fever consists; and hence the question arises how, upon the grounds we have assumed, can the origin of such lesions be explained. Now, undoubtedly in certain cases such complications may arise from the absorption of inflammatory, putrid, and other noxious secretions from the maternal passages, as also from causes tending generally to vitiate the blood—but not necessarily or exclusively; for, through the influence of the nervous upon the vascular system, such inflammatory lesions may arise from the operation of various other causes acting upon the nervous centres, and through these exciting and maintaining a state of preternatural activity of the heart and circulation, in which it will happen that the vessels of the uterine organs and the capillary, rather than the larger, will yield to the increased force of the circulation, allow themselves to become abnormally distended, and so the phenomena of inflammation to arise. Thus, in one series of cases we observe puerperal fever to occur in connexion with irritative disturbance of various organs—such as the mammary glands, the intestinal canal, the brain, or the spinal cord; and such irritative disturbance may, through the influence referred to, adequately give rise to fever, with all the attendant lesions which characterize the puerperal. For inasmuch as all causes of irritation, wherever existing, are immediately felt in the central parts of the nervous system, and through the sympathies subsisting between it and the vascular, tend to excite the latter to preternatural action, it will follow, on such febrile action taking place, that the vessels of any organs that may have been relatively weakened by antecedent actions or other causes, will yield more than others to the general impulse of the blood, allow themselves to become abnormally distended, and thus inflamed. Now, such debility, and consequent disposition to inflammation, is common to the vascular system of the uterus and its appendages after labour, as a consequence of the antecedent actions of pregnancy and parturition, and those necessarily going on in the uterine system subsequently to childbirth; and hence it is that they are so prone to yield preternaturally, and to become the seat of inflammation, in any febrile movement that may be casually excited after labour. In another series of cases, we observe puerperal fever to arise from the operation of various causes upon the nervous system, of a general and non-specific character—such as over-fatigue, over-excitement, and exposure to cold; and here, as in the former case, a febrile movement being excited through the sympathy subsisting between the nervous and the vascular systems, the uterine vessels may allow themselves to become abnormally distended, and so the first stage of inflammation to be established. In a third series of cases, we observe puerperal fever to arise from the direct consequences

* An Account of some of the most important Diseases Peculiar to Women, pp. 1–2.

of mechanical injury, such as contusion or mechanical injury of the uterine organs during labour; and here the nervous and the vascular systems are incited to preternatural and abnormal action through the medium of inflammatory irritation acting upon the peripheries of the nervous system. And lastly, it is not to be denied that puerperal fever is often traceable to the operation of causes which directly tend to vitiate the blood—such as various epidemic and endemic influences, noxious miasmata, specific contagions, and unhealthy discharges absorbed from the maternal passages, which, equally with the former, may give rise to fever, with all the attendant lesions which distinguish the puerperal. These causes, however, equally with the others, act primarily upon the nervous centres, and establish the phenomena of fever and inflammation through the medium of the sympathy subsisting between the nervous and the vascular systems; and here, as in the former case, the capillaries of the uterine system are predisposed to, and become the seat of, inflammation as a consequence of the antecedent actions of pregnancy and parturition, and the necessary changes consequent upon labour.

Such, to our mind, are some of the several ways in which puerperal fever may arise; and if our views are correct, it must follow that it cannot in all cases be referred to any one cause, however powerful or frequently in operation, even as that of vitiated blood. From general causes acting upon the nervous system, we may deduce not only the occurrence of fever, but of fever attended with the most prominent lesions which distinguish the puerperal. And with regard to minor differences in the type of the disease, as observed in different persons and under different circumstances, we may observe that they are often capable of an explanation by a reference to the intensity of the operation of the same cause, and the peculiarities of individual constitutions, rather than by any essential difference in the nature of the exciting cause. Thus, the more powerfully the nervous system is injured or impaired by the offending cause of the attack, the greater will be the prostration and the less marked the reaction; and hence, in such cases, fever of varying type rather than inflammation will be the consequence; whilst, on the other hand, the less powerful the injurious impression on the nervous system, or the greater the resisting power of the individual, the more decided will be the reaction, and the greater the tendency to inflammatory complications. Moreover, in proportion as particular parts of the capillary system are prone to yield, from antecedent weakness or other causes, to the reactive force of the circulation rather than the capillaries generally, will the disease partake of the inflammatory rather than the febrile type. Bilious, cerebral, gastric, pulmonary, and other complications, would appear to have reference to the predisposition of particular organs to morbid action, consequent upon antecedent weakness or derangement. Persons, for instance, naturally subject to bilious derangements may be supposed to have the hepatic functions more readily disordered than any others, under the operation of a general disturbing cause; and so the occurrence of antecedent disease or debility of any organ—pulmonary, gastric, or cerebral—will predispose such organ to inflammatory action in the course of any febrile movement which may be casually excited: it being an admitted physiological fact, that the weakest part feels most that which affects the whole; and hence it happens that the capillary vessels of organs relatively weak

yield most to the force of the circulation, under the influence of any such febrile or preternatural excitement.

The length at which we have entered upon this question forbids our noticing in detail the various causes by which the blood may be infected or vitiated after childbirth, so as to give rise to some of the more specific types of puerperal fever. Dr. Simpson is of opinion that the infecting cause is generally, if not always, an inflammatory secretion, just as is the inoculable matter of small-pox, cow-pox, and syphilis; and he argues against its being a mere product of putrefaction. We regard this, however, as a too partial and limited view of the nature of the causes from which epidemic, endemic, and some of the more specific forms of puerperal fever take their rise. These we believe, on the contrary, to be dependent upon the introduction into, or generation in, the system of poisonous principles evolved from organic matter in certain states of retrograde decay. We admit that all types and forms of decay do not furnish the specific products upon which the origin and dissemination of specific forms of fever and disease depend. But decay and putrefaction are but generic terms applied to various retrograde changes in the elements of organic matter, and are liable to be variously modified, retarded, or accelerated by a variety of circumstances. Moreover, regarded as the grouping of the atoms of organic matter into simpler and less complex combinations, we can readily conceive how different groupings of such atoms should be possessed of different physical and physiological properties and effects upon the living body. Let us revert, in illustration, to the different physical and physiological properties of oxalic acid and sugar, of otto of rose and sulphuretted hydrogen, of oil of turpentine and essence of lemons—and yet how similar their elementary composition! Now, if in these cases the same atoms differently blended yield products so manifestly different, it is easy to understand how the very complex atoms of organic matter, in the course of their retrograde metamorphoses, should, under the influence of various physical causes, enter into different combinations, some of which may be noxious, and others innocuous, to the human body. Thus it is notorious that, whilst the products of organic decay generally are not the specific cause of typhus, that typhus nevertheless prevails most commonly where such decay abounds; and so it is with cholera and other zymotic diseases: from which it would appear probable that the products of some particular, or perhaps specific, form of decay were their causes respectively. And inasmuch as the blood constantly teems with organic matter in a state of retrograde change, it is evidently possible that, under the influence of certain atmospheric constitutions or conditions, pernicious atomic groupings may take place within the body when the normal force tending to resist such changes is inadequate to prevent them; and hence that the causes and phenomena of specific fever, whether puerperal or otherwise, may be sporadically developed. Inflammatory products in some cases, and putrefactive in others, being equally possessed of noxious properties, and such as received into, or generated in, the organism, and applied through the medium of the blood to the nervous centres, may produce the phenomena of irritative, inflammatory, and febrile disturbance—each assuming more or less specific types or characters, according to the peculiar properties of the poisonous principle in operation.

We proceed to a consideration of the papers On Puerperal Arterial Obstruction and Inflammation, and On Tetanus following Lesions of the Uterus; and here we may observe, that little is left us to offer but an analytical notice—the subjects treated of being equally novel and original, and such as have hitherto received but little notice from the profession in relation to the puerperal state.

Inflammation and obstruction of the arteries in the puerperal female would appear to be producible by a variety of causes, and more particularly the following:—1. By the separation of old or organized cardiac vegetations, and their subsequent transference into the arterial canals, as more particularly pointed out by Dr. Kirkes, Virchow, and others; 2. By the passing forward into the current of the circulation of recent fibrinous masses formed in the cavities of the heart or larger arterial vessels; 3. By local arteritis; 4. By laceration of the internal coats of the occluded vessels; and 5. By morbid materials carried from the systemic venous system, and lodged in the pulmonary artery or its branches.

In illustration of the first mode of causation, 5 cases are reported of arterial obstruction, in all of which vegetations were found on the aortic valves; and loose bodies, having the same physical appearance and structure as the cardiac vegetations, were discovered on the obstructed arteries. It is argued, that the cause of arterial obstruction in these instances could not be local arteritis, inasmuch as the symptoms of arterial obstruction occurred suddenly and almost instantaneously, and the obstructed artery in some exhibited no post-mortem evidence of thickening, or previous inflammatory disease. And further, it is contended that the separation of vegetations from the heart is rendered highly probable by a variety of considerations: for, 1, The vegetations, whether sessile or pediculated are often loosely attached, being easily removed after death by the handle of the scalpel; 2, The valves to which they are adherent are parts constantly in motion; 3, Currents of blood are ever rushing over them with considerable force; and 4, When once separated, they will be carried along until, meeting at last with a vessel whose calibre is smaller than their bulk, they become impacted; or they may become arrested where a larger vessel divides into the branches, each of which is smaller than the detached vegetation. The second cause of arterial obstruction is that in which recently-formed coagula are projected from the heart into the general circulation. Fibrinous polypi have been found after death in the cavity of the heart; and it is observed that they are specially likely to be formed in the interior of the left ventricle when the latter is anywhere mechanically rough or irregular, as from the presence of globular polypi in its cavity, vegetations on the valves, or endocarditic inflammation of its lining membrane, and more especially when the blood is super-fibrinated, as happens in the puerperal state. In illustration of the third cause of obliteration of arteries in puerperal patients—that arising from local arteritis—a case is referred to as having occurred in the practice of Dr. Duncan, in which acute gangrene of both lower extremities had come on in a patient who had been confined only two weeks. On dissection, no disease was discovered in the heart, its walls, valves, or cavities. But the aorta was found blocked by a firm fibrinous exudation, which descended along the iliac arteries, and in some situations

was closely adherent to the arterial walls, the coats of the obstructed arteries being at the same time much thickened. The *fourth* cause of arterial obstruction in puerperal patients is laceration of the internal coats of the artery,—a subject which is illustrated by a reference to the writings of Dr. Hodgson and Professor Turner, and a case published by Dr. Oke, of Southampton. In this the patient had uterine hæmorrhage, terminating in abortion. Three days afterwards, her left arm had become cold and insensible, and the tips of the fingers discoloured. No pulsation could be felt in the limb. The action of the heart and the respiration were natural. The tips of the fingers became gangrenous, and dropped off, but the gangrene proceeded no further, and the arm recovered its natural plumpness. The patient is still alive, and no heart affection can be detected. The fifth cause of puerperal arterial obstruction is a diseased condition of the blood, tending to obstruction of the pulmonary artery and its branches by morbid materials coming from the systemic venous circulation, and passing through the right side of the heart. In support of this doctrine, a variety of cases are given in which obstruction of the pulmonary artery was met with in connexion with and as a sequence of phlebitis and phlebitic concretions in the uterine, pelvic, or other systemic veins. Other cases are, however, given, in which no antecedent venous inflammation existed, and it is surmised that some cases of sudden and unexpected death in the puerperal mother may have its origin in this pathological condition. With regard to the general causes capable of giving rise to arterial obstruction and inflammation in the puerperal state, the author is inclined to refer them to certain abnormal peculiarities in the blood of the puerperal female, such as the redundancy of fibrin common to this period, the amount of effete matter thrown into it, consequent upon the retrograde metamorphoses or disintegration of the uterus, and the new materials formed in it for the formation of the milk. These constitute a state of blood, it is argued, which, under the influence of a variety of accidental causes, inducing fever or interrupted excretion, may favour the development of arterial obstructions. The symptoms of such lesions will of course vary according to the artery obstructed, and with the function of the part to which the artery belongs. Hence the results of arterial obstruction are very different, according as the occluded artery belongs to organs connected with the head, chest, or abdomen, or is an artery belonging to one of the extremities of the body. Our knowledge of the former series of cases is limited to the possible occurrence of symptoms of paralysis, loss of vision, and *ramollissement* in regard to the brain, and distressing disturbance in the actions of the heart and lungs, with possibly gangrene of the latter organs in regard to the lungs when the arteries respectively of the brain and lungs are concerned. With regard, however, to arterial obstructions of the limbs, the following symptoms are liable to occur in the affected extremity:—1. Arrest of the pulse below the site of obstruction. 2. Increased force of pulsation in the artery above the site of obstruction. 3. Fall in the temperature of the limb. 4. Lesions of the motor and sensory powers in the limb in which the artery has become obstructed, giving rise to paralysis, neuralgia, &c. 5. Gangrene below or beyond the seat of arterial obstruction.

The foregoing must be regarded as a very condensed summary of the principal point mooted in one of the most original and interesting papers which has recently been published in obstetric pathology; and we cannot doubt but that it will prove the forerunner of many valuable investigations on the same subject. On the present occasion any critical remarks would be altogether out of place; but we cannot help directing attention to the light thrown by this communication upon the kindred subject of obstruction and inflammation of veins. Thus, it would appear that of the five modes in which obstruction and inflammation of arteries are alleged to take place, in one only are they assumed to depend upon idiopathic inflammation of the coats of these vessels; and this doctrine is so little supported by the circumstances of the case adduced in support of it, that additional evidence will be required before it can be admitted by the profession. In nearly all the other modes the obstructing cause is shown to be some morbid condition or constituent of the blood; and such was the doctrine affirmed by Dr. Mackenzie, after a lengthened investigation, in regard to the causation of the obstruction and inflammation of veins;* nay, further, the several conditions referred to by him as being the most favourable to the production of such lesions of the veins in the puerperal state, are precisely those alleged by Dr. Simpson to be the most powerful in the production of the same lesions of the arteries. So far, then, the researches of Dr. Mackenzie and those of our author may be regarded as mutually supporting each other, and point clearly to the direction in which further investigations may most profitably be made.

The researches of Dr. Simpson have led him to the conclusion, contrary to general belief, that internal injuries or lesions of the uterus, both in the unimpregnated and puerperal states, are sometimes followed by tetanus in an acute and fatal form; and he refers to a series of twenty-five cases as showing that traumatic tetanus does supervene occasionally as a secondary obstetrical disease in the same way as all medical authorities acknowledge it to supervene occasionally; and still more frequently as a secondary surgical disease. These cases tend to show that tetanus may follow—1, lesions of the unimpregnated uterus, as well as the lesions left in the uterus and maternal canals; 2, by abortion; and 3, by parturition at the full time. The nature, causes, and treatment of tetanus thus supervening are the subject of some interesting remarks which are appended to the cases. Referring to the general fact, that the existence of an injury or wound upon the external parts of the body is by far the most common cause of tetanus, it is affirmed that a similar state of lesion exists upon the interior of the uterus—viz., that caused by the separation of the decidua and the rupture of the organic attachments of the placenta to the uterus. Hence it is remarked, that obstetrical tetanus has in this respect an exciting cause essentially similar to surgical tetanus; and that the reason that this state of lesion of the interior of the uterus does not more frequently give rise to tetanus, is simply that the organ is chiefly supplied with nerves from the sympathetic system;

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tetanus being an affection far more easily excited by lesions of parts supplied with nerves from the cerebro-spinal system, than by lesions of parts supplied with nerves from the sympathetic system. Further, it is argued that as we have in obstetric pathology evidence almost amounting to certainty that the analogous super-excitable state of the cerebro-spinal system of nerves, which gives rise to eclampsia or puerperal convulsions, is generally produced by the existence of a morbid poison in the blood, it is possible that the generation of a special blood poison at the site of the wound or elsewhere, may sometimes in the same way give rise to obstetrical and surgical tetanus. It is worthy of remark, that in some of the cases quoted the tetanic attack followed upon exposure to cold, a common exciting cause of ordinary tetanus. Hence it may be concluded that the more immediate causes of obstetrical tetanus are, 1, the uterine lesion following abortion and parturition; 2, a morbid condition of the blood consequent upon the absorption of a special blood poison generated at the site of the wound; and 3, exposure to cold, or rather to currents of cold and damp air, especially if the person immediately before this exposure had been over-heated or perspiring. With regard to treatment, it is observed that no kind of local treatment to the seat of the original uterine lesion could be well applied, or would probably be of any avail if applied; and as to constitutional means, that the following are probably the most important:

1st. The greatest possible quietude and isolation of the patient from all irritation, corporeal or mental, during the course, and for some time even after the resolution, of the disease.

2nd. The special avoidance of painful and generally impracticable attempts at opening the mouth in order to swallow, but sustaining the strength of the patient, and allaying thirst, by enemata, or by fluids applied to the general surface of the body.

3rd. If there is any well-grounded hope of irritating matters lodged in the bowels acting as an exciting or aggravating cause, to sweep out the intestinal canal at the commencement of the disease with an appropriate enema.

4th. To relax the tonic spasms of the affected muscles, and diminish the exalted reflex excitability of the spinal system, by sedatives or antispasmodics; with the prospect of either directly subduing this morbid reflex excitability, or of warding off the immediate dangers of the disease, and allowing the case to pass on from an acute and dangerous attack to a subacute and far more hopeful and tractable form of the malady.

In connexion with this latter indication, the author refers to the employment of various sedatives and antispasmodics; and after pointing out the inutility of most of them, he speaks more hopefully of the antispasmodic action of chloroform sustained for many hours or even days. He dwells upon the safety of its continued employment, and gives a case which occurred in the practice of Professor Laurie, of Glasgow, in which it was successfully exhibited.

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and physiology than practical midwifery: as, for instance, the very elaborate essay On Hermaphroditism, which occupies 141 pages, and others On the Diseases of the Placenta, which have been fully treated of by Dr. Barnes in some recent numbers of this Journal,—we come to a series of papers On the Pathology of Infancy and Childhood, of which one on the external use of oil in the prevention and treatment of scrofula and phthisis is probably the most interesting. We shall examine this paper less with reference to the special curative powers of oil thus externally applied in these affections, for this appears at present to be a question *sub judice*, than its employment generally as a therapeutical agent, the value of which, in various cases, we have had many opportunities of observing.

Among the nations of antiquity, as in some Eastern countries at the present day, the external application of oil to the human body formed an important item of the medical art. By the former it was used both as a hygienic and curative agent, and the rules by which its application was regulated constituted a distinct branch of healing, known as iatraleptic medicine. The writings of Aretæus, Celsus, Pliny, Galen, and others, contain many passages in which its efficacy is spoken of; and among other remedies, we find it recommended in various diseases—such as fevers, eruptive diseases, gout, palsy, lethargy, tetanus, cholera, melancholy, dropsy, &c. The sacred writings also contain various references to this practice, so that we cannot doubt that in ancient times considerable importance was attached to it. The same would appear to be the case among many Eastern nations at the present day, in which it forms an almost necessary concomitant or adjunct of the bath, and is supposed to be possessed of many healing virtues. Among others, maintaining a moist state of the skin, lessening the irritation produced by acrid perspiration, and preventing an excessive transpiration of aqueous matter from the body. The reader will find many interesting remarks on this subject in a paper published by Mr. W. Hunter, in the second volume of the 'Edinburgh Journal' for 1806, the perusal of which originally led us to make a trial of the practice; from which we are enabled to speak favourably of it in many cases, and more particularly the following: 1st. Febrile affections generally, in which the skin is either preternaturally dry or morbidly suffused with perspiration. 2nd. The remittent fevers of children connected with irritative, congestive, or inflammatory conditions of the gastro-intestinal mucous membrane, and those which are so apt to follow some of the eruptive fevers, such as measles and scarlet fever, upon exposure to cold, or any casual interruption of the cutaneous functions. 3rd. Diseased states of the skin of a scaly character, such as the several forms of psoriasis and lepra, in which, with the addition of a little liquid tar or creosote, the external application of oil is signally beneficial. Lastly, strumous affections generally, and more especially those in which the mesenteric glands are obstructed, and in which the skin for the most part is either morbidly dry or preternaturally relaxed. In these and some other morbid states of the economy we have personally witnessed the beneficial effects of the innunction of oil, and we are therefore disposed to receive with considerable confidence the evidence of Dr. Simpson in support of its prophylactic and curative powers in consumption and scrofula.

The origin and results of his investigations may be thus briefly stated. When on a professional visit, in October, 1852, to Galashiels, in Roxburghshire, his attention was incidentally directed by Dr. Macdougal to the healthy state and robust appearance of the operatives at the large woollen manufactories in that town, and to the fact that they were strikingly exempt from consumption and scrofula—an exemption which they attributed to the free external application of oil to their bodies, which occurred in various parts of the manufacture of woollen fabrics. This casual observation appeared to him so interesting in itself, and possibly so important in the consequences to which it might lead, that it seemed to him a matter of moment to ascertain, *first*, if the same relative immunity from phthisical and strumous disease had been observed among the workers at other woollen factories in Scotland; *secondly*, if this immunity were attributable to the external application of oil; and *thirdly*, if the employment of external inunction, when resorted to as a prophylactic or therapeutic means, were capable of acting beneficially upon the body, and could be applied practically in the prevention and treatment of consumption, scrofula, and other affections. We cannot enter at length into the evidence adduced on the subject of these several questions, but may observe that it is tolerably conclusive as to the fact that the operatives in large woollen manufactories are remarkably exempt from consumption and scrofula, and that this exemption is regulated by the more or less "oily" nature of the departments of work in which they are engaged in the mills; so that they in general markedly improve in appearance and health when set to work at the more oily processes, and often as markedly decline after leaving them. With regard, however, to the prophylactic and curative powers of systematic oil inunction as a medicinal measure in tubercular disease, we can only repeat that at present this is a question *sub judice.* No cases are given by Dr. Simpson which support this conclusion, and all that can be said is, that he has used many ingenious arguments in its favour. Time and further experience must determine this question; and as it is one of considerable importance, we venture to append the rules given by Dr. Simpson for external oil inunction, with the propriety of which our own experience leads us entirely to concur:

"The oil selected ought to be bland and inodorous, like olive or salad oil, and it should be applied moderately warm. Its application is thus rendered far more agreeable to the feelings of the patient; the danger of chills is avoided; and the act of absorption is increased by an elevated temperature. 2. A considerable amount and duration of friction should be used either by the patient or his attendant, or by both, in order to rub in the oil as much as possible, and thus promote the completeness of its absorption. 3. The oil and friction should be applied to the whole cutaneous surface of the trunk and extremities, but especially to those parts of it where the skin is thin and the function of absorption greatest, as the sides, the flexures of the limbs, the insides of the thighs, &c. 4. The average quantity of oil requiring to be used at each inunction is about a large wineglassful. 5. In cases in which it is an important object to introduce the oil into the system as freely and rapidly as possible, the inunction of it may be practised twice or oftener in twenty-four hours, especially with children; but the best time for a single daily oil inunction is immediately before retiring to bed, as the imbibition of any free oil left on the surface may afterwards go on during the night; and to save the bed-clothes, the patient should sleep in a dress of flannel, linen, or other material that stretches beyond the feet. 6. In order to maintain the full absorb-

ing action of the skin in conjunction with the practice of oil inunction, occasional warm sponging or bathing of the whole cutaneous surface with tepid water, or with a weak solution of soda and water, should be employed, either immediately before an inunction or several hours subsequently to one. 7. It is to be remembered that the cutaneous absorption of oil is usually, though not always, comparatively more slow and difficult, and hence the practice itself is so far more disagreeable for two or three weeks after the inunction is first begun than subsequently; and consequently that less oil disappears and more friction is required in the beginning of the practice than afterwards."

We come, in the next place, to the series of papers On Anæsthesia, which form so considerable and important a constituent of the volume, and with which, more than perhaps anything else, the name and reputation of our author are identified. Of these papers generally it will be sufficient to observe, that whether regarded with reference to their historical, physiological, or obstetrical value, they will ever possess the highest interest, and claim the attention of the profession, not only as portraying a new phase in obstetric practice, but as containing also the history of the discovery and introduction of the best anæsthetic agent with which we are acquainted. With many of the subjects treated of in this series, however, we have little to do in the present article. The objections which formerly existed to anæsthesia in midwifery have, in a great measure, passed away; numberless facts have attested its value and safety, and the only question now remaining to be considered is the best means of averting the occasional dangers with which its employment is attended. It is to this alone that we shall address any observations which we have to offer upon the subject, and in doing so will leave out of consideration the many speculative views with which it has been encumbered, confining ourselves, as far as possible, to a brief statement of our own practical experience in regard to it.

Death from chloroform has been affirmed to take place in a variety of ways—from coma, or a suspension of the nervous and sensorial functions generally—from asphyxia, or a primary suspension of the respiratory functions,—from syncope, or a primary suspension of the heart's action. We do not doubt that it *may* occur in each of these ways, but in a practical point of view it is necessary to distinguish between the more ordinary and the more exceptional modes in which it may take place; because a knowledge of each is important to a right understanding of the precautionary and curative measures to be adopted. Now in ordinary cases, in which there is no peculiar idiosyncrasy of the patient, and no undue haste or rapidity in the administration of the agent, we apprehend that the sequence of events tending to fatal anæsthesia is the following:—1st. A suspension of the functions of the ganglia related to common or general sensibility; 2ndly. A suspension of those related to volition; and 3rdly. A suspension of those related to the respiratory movements. The functions of sensation, volition, and respiration being successively annulled in the order here stated. Such, we apprehend, in ordinary cases, is the progressive tendency of anæsthesia to a fatal result; and if so, it points to the importance of administering the drug very moderately in the first place, and carefully watching the respiratory movements in the second. In certain exceptional cases, however, death may take place in one of the following ways—first, from a sudden and injurious impression

upon the brain and nervous centres, simultaneously annulling the functions of respiration and circulation, together with those of sensation and volition, as in the case of violent nervous shock or concussion. In some such cases this result may be due to the concentrated manner in which the chloroform is inhaled; but in others it would appear to be connected with a peculiar idiosyncrasy or susceptibility of the patient or her nervous system; and we have more particularly observed it in weakly anæmic females. One case will illustrate this peculiarity. An extremely anæmic and highly sensitive young lady expressed a desire to take chloroform in the course of her first labour. Not more than a teaspoonful was put upon a handkerchief, and this was placed at a moderate distance from her face to be inhaled. Notwithstanding these precautions, however, she fell back in a state of alarming syncope almost immediately after the first inhalation; both respiration and the heart's action had ceased; her face was deadly pale, and some seconds elapsed before animation returned. Now it is impossible, we conceive, to doubt, on the one hand, that in this case the temporary suspension of the respiratory and circulatory functions—which, if prolonged, would have proved fatal—was due to the sudden impression of the vapour of chloroform upon the nervous centres; or, on the other, that this was intimately connected with some peculiar idiosyncrasy of the patient, such as we have referred to. In other cases, however, we have reason to believe that death may commence primarily at either the heart or the lungs. In the former case, by syncope, from a kind of paralysis of the organ, occasioned by the circulation of the vapour of chloroform through the coronary vessels; in the latter, by asphyxia, induced either by the pungency of the vapour exciting irritation or spasm of the glottis, and so preventing the entrance of air into the lungs, or from the air admitted being so highly charged with the vapour of chloroform as to be irrespirable, or prevent, by its high specific gravity, the exosmosis of carbonic acid from the blood. These cases, however, we regard as the exceptional rather than the usual modes in which death takes place. They would moreover appear to be occasioned by the incautious use of the agent, and are attended with the manifestation of symptoms, such as struggling, suffusion of the face, &c., which, if properly attended to, would enable us to avert serious consequences. We repeat, it is our conviction that in the great majority of cases in which death follows the administration of chloroform, it is due to the suspension of the respiratory functions through the narcotic or benumbing influence of the agent upon the brain and nervous centres, and more particularly upon the ganglia immediately subservient to respiration; and we would add, that we are supported in this opinion by the following facts:—1st. That in a great number of experiments and observations made upon the lower animals, the heart was found to be irritable and contractile for some time after respiration had ceased; and 2ndly. That many were restored to life from an apparently hopeless state of suspended animation by steadily and perseveringly maintaining the respiratory movements by rhythmical compression of the chest. Nay, more, it fell to our lot to have occasion to test the value of this proceeding in the case of a lady in whom respiration and the heart's action had both stopped from the incautious administration of chloroform, and with the most

perfect success. Our practical conclusion, therefore, would be, that safety in the administration of chloroform is to be mainly ensured by giving it, in the first place, most moderately, cautiously, and well diluted with atmospheric air, until the nervous system has, as it were, become accustomed to its influence; and 2ndly. By observing closely its action upon the brain and nervous system, the heart's action, and the respiration, until we are satisfied that no peculiar idiosyncrasy exists on the part of the patient to endanger its employment. Assured upon these points, the next precautionary measure to be adopted is to watch closely the respiratory movements during the further use of the agent, as the most certain key to any threatened danger, resorting immediately to artificial respiration whenever the function appears to be embarrassed or suspended. As already stated, we believe that this can be best accomplished by the persevering employment of rhythmical compression of the chest, so that the contained air may be expelled on compression, and a fresh supply drawn in by the elastic recoil or expansion of the walls of the chest when the compressing force is removed. This proceeding requires at least no accessory or complicated apparatus; it can be instantly resorted to; and looking to its success in the several cases in which we have tried it, we have great confidence in recommending it to the notice of the profession.

With these observations we conclude our notice of the second volume of Dr. Simpson's obstetric works. It has been necessarily partial and fragmentary; but in making our selection of the topics to be discussed, we have dwelt chiefly upon those which were of a practical nature, and therefore most likely to interest the practical reader. It would be impossible, in an article like the present, to give anything like a complete exposition of the many subjects treated of in it; and we must therefore refer our readers to the work itself for a just appreciation of the many interesting facts and laborious researches which are embodied in its pages. It is difficult to over-estimate the value of such writings, whilst we are unwilling, at the same time, to indulge in the language of flattery or adulation; but dispassionately considered, and impartially estimated, we venture to believe that their intrinsic merits are such as will carry their author's name down to the latest posterity as one of the most zealous and indefatigable cultivators of the obstetric art.

Review V.

1. *Report of the Committee for Scientific Inquiries in relation to the Cholera Epidemic of* 1854. Presented to both Houses of Parliament by command of her Majesty. 8vo, pp. 129. *Appendix to Report of the Committee for Scientific Inquiries in relation to the Cholera Epidemic of* 1854.—*London.* 8vo, pp. 322.

2. *Report of the Commissioners appointed to Inquire into the Causes which have led to, or have aggravated, the late outbreak of Cholera in the towns of Newcastle-upon-Tyne, Gateshead, and Tynemouth.* Presented to both Houses of Parliament by command of her Majesty.—*London,* 1854. Folio, pp. 580.

3. *Report made by Dr. Milroy to the Colonial Office on the Cholera Epidemic in Jamaica*, 1850–51, &c. Ordered by the House of Commons to be printed, May 11th, 1854.—*London.* Folio, pp. 147.

4. *Report on the Cholera which attacked the Fleet in the Black Sea in August*, 1854, *more particularly as relates to her Majesty's ships "Britannia," "Albion," and "Trafalgar."* Printed by order of the Right Hon. the Lords Commissioners of the Admiralty.—*London.* Folio, pp. 15.

5. *Reports relating to the Sanitary Condition of the City of London.* By JOHN SIMON, F.R.S., Surgeon to St. Thomas's Hospital, and Officer of Health to the City of London.—*London*, 1854. pp. 312.

6. *Report on the Cholera Epidemic of* 1854, *as it prevailed in the City of London.* By JOHN SIMON, F.R.S., Medical Officer of Health to the City of London, &c.—*London*, 1854. pp. 18.

7. *Third Annual Report of the Commissioners for Administering the Laws for Relief of the Poor in Ireland under the Medical Charities Act*, 14 *and* 15 *Vic. cap.* 68. Presented to both Houses of Parliament by command of her Majesty.—*Dublin*, 1855. pp. 381.

8. *Report of the Commissioners of Health, Ireland, on the Epidemics of* 1846 *to* 1850. Presented to both Houses of Parliament by command of her Majesty.—*Dublin*, 1852. pp. 79.

9. *Appendix* (C.) *to the Report of the General Board of Health on the Epidemic Cholera of* 1848 *and* 1849. *Abstract of Report by* JAMES WYNNE, M.D., *on Epidemic Cholera, as it prevailed in the United States in* 1849 *and* 1850. Presented to both Houses of Parliament by command of her Majesty.—*London*, 1852. pp. 93.

10. *Report upon the Sanitary Condition of New Orleans.* By E. H. BARTON, M.D. (Forming an Appendix to *Report of the Sanitary Commission of New Orleans on the Epidemic Yellow Fever of* 1853. Published by authority of the City Council of New Orleans.) pp. 249.

11. *Untersuchungen und Beobachtungen über die Verbreitungsart der Cholera, nebst Betrachtungen über Massregeln, derselben Einhalt zu thun.* Von Dr. MAX PETTENKOFER, Professor der Medizinischen Chemie an der Universität zu München.—*München*, 1855. pp. 374.
Investigations and Observations on the Propagation of Cholera; with Remarks on the Precautions necessary to Arrest its Progress. By MAX PETTENKOFER, Professor of Medical Chemistry at the University of Munich.

12. *Report on the Cholera Outbreak in the Parish of St. James's, Westminster, during the Autumn of* 1854. Presented to the Vestry by the Cholera Inquiry Committee, July, 1855.—*London*, 1855. pp. 175.

13. *Report on the last Two Cholera Epidemics of London, as affected by the Consumption of Impure Water.* Addressed to the Right Hon. the President of the General Board of Health, by the Medical Officer of the Board.—*London*, 1856. pp. 35.

14. *Report on the Outbreak of Epidemic Cholera in Brecon in* 1854. By PRESTWOOD LUCAS, M.D.—*London*, 1855. pp. 27.

15. *On Animal Decomposition as the Chief Promotive Cause of Cholera.* By HENRY HARTSHORNE, M.D. (From the 'Medical Examiner,' August, 1855.)—*Philadelphia*, 1855. pp. 12.

16. *Memoir on the Cholera at Oxford in the year* 1854. By HENRY WENTWORTH ACLAND, M.D., F.R.S., F.R.G.S., Physician to the Radcliffe Infirmary, and Lee's Reader in Anatomy in the University of Oxford, &c. — *London*, 1856. 4to, pp. 172.

IN a former article* we proposed to inquire into the history and origin of cholera, with the twofold intention of placing prominently before our readers those facts in its history which seem to indicate an indigenous rather than an exotic origin to this pestilence, and of pointing out the circumstances under which it has prevailed. Upon the subject of contagion we did not enter, because its propagation by means of human intercourse, in the manner in which small-pox, scarlatina, or measles are propagated, has never been generally believed, and its history is adverse to the supposition that its spread as an epidemic is in any considerable degree, if at all, attributable to human intercourse. After stating reasons which lead us to infer that cholera is not altogether new to Great Britain, and adducing some instances which seem to prove that it has, at least occasionally, arisen irrespective of the introduction of a poison from abroad, we next proceeded to inquire into the circumstances under which epidemic outbreaks have occurred. These were found to be referrible to two chief heads: "seasonal or meteorological conditions," and "localizing causes." Of these necessary factors in the causation of cholera, termed by Dr. Barton, the "two blades" of the shears of fate," we had space only for the consideration of the former. As regards this country, it appeared that certain meteorological phenomena which, in the aggregate, borrowing an idea from the older physicians, we termed the pestilential constitution of the year, have mostly accompanied outbreaks of cholera. Allowing for difference of climate and situation, it was also found that the atmospheric conditions under which cholera has usually prevailed abroad, have been almost identical. These conditions were found to be, a somewhat variable but elevated temperature, a still and peculiarly oppressive state of the atmosphere—more oppressive than the simple elevation of the thermometer can account for—conjoined with a certain degree of moisture. Such climatic conditions are rarely, if ever, confined to a limited locality. Situation may aggravate them; lowness of level, or the ill-arrangement of streets or blocks of buildings may add to their force; but in general, the result of such local circumstances upon local climate excepted, when these seasonal conditions exist in one place, they must be likewise present in many others. Yet of such places as partake in the same seasonal and meteorological influences, some usually escape an epidemic visitation at the very period when others in the immediate vicinity are suffering severely from its presence. Even in the same town, whilst the inhabitants of some streets or courts are being decimated, those dwelling in others, not far distant altogether escape; or, as frequently happens, the inmates of certain houses suffer severely, whilst their neighbours are entirely spared.

* British and Foreign Medico-Chirurgical Review, vol. xvii. p. 286.

Thus, from Mr. Simon's Official Reports to the City Commissioners of Sewers, we learn that the cholera epidemics of 1849 and 1854 fell with unequal force upon different localities.* For example, the cholera-mortality in 1849 was 19 in the 10,000 in the north-west sub-registration district of the City of London Union, and 47 in the 10,000 in the Cripplegate sub-registration district. These districts are at the same level with each other, and with the adjoining Hackney-road division of Bethnal-green, in which the mortality was as high as 110 in each 10,000 persons.† A minuter investigation shows still more remarkable differences; for in certain districts of the City the mortality from the several species of alvine flux was in the same year extremely small, in others excessively large. Thus, in Cordwainers', Coleman-street, and Aldersgate-within Wards, out of resident populations estimated as amounting to more than 7800 persons, there occurred only 4 deaths from cholera; whilst in a band of two or three hundred yards' width northwards from Blackfriars Bridge, "in the parallelogram which lies along the main road from Stonecutter-street to Bridewell Hospital, were 76 deaths; . . . in the little clump of houses forming the angle of Farringdon-street and Holborn-hill, were 17 deaths; . . . in a square space behind twenty-seven shopfronts in Fleet-street were 57 deaths; . . . . and lastly, in the small parish of St. Ann's, Blackfriars, the deaths were at the rate of 25 to every thousand of its population." The mortality from cholera in the City in 1854 varied from 8·90 per 10,000 inhabitants in the north district of West London, and 8·57 per 10,000 in the north-east division of the City Union, to 23·32 per 10,000 in Cripplegate. Differences these sufficiently remarkable, and evidently not referrible to the epidemic constitution of the atmosphere alone, but to be explained only by the presumed existence of some special circumstances in the localities themselves or their inhabitants. Moreover, seasons presenting all the characters which conjointly form what we have termed the pestilential constitution,‡ have without doubt existed very often when there has been no accompanying pestilence. During the century and a quarter that England was free from epidemic pestilences, many such seasons must have occurred; and in tropical climates they exist in ordinary years. In his fourth recapitulatory proposition, Dr. Barton says that the atmospherical cause of pestilence is annually more or less present at New Orleans, yet neither yellow fever nor cholera are annual visitants to that city. Another co-efficient, at least, is therefore required in order to give character and energy to the seasonal conditions which favour the development of cholera. This is what has already been alluded to as the terrene element of Dr. Barton, and corresponds with what have been termed the localizing causes of cholera. That it is strictly local is further evidenced by the fact that an analysis of the history of cholera-epidemics shows them to be most frequently made up of a succession of partial local outbreaks, and this not only as regards different districts, but even the same place. On the other hand, it has often occurred that the pestilence has lingered in some few favourite haunts throughout the entire course§ of an epidemic; and

* Reports, pp. 231, 167, and 96.
† Registrar-General's Report on the Mortality of Cholera in 1848–9, p. clxviii.
‡ British and Foreign Medico-Chirurgical Review, vol. xvii. pp. 296–7.
§ See Dr. Acland's Memoir, p. 29.

now and then, after visiting a place at the commencement of a visitation, it has returned to it again, after an interval of complete immunity, before its close.

The tendency of cholera to return at a subsequent visitation to the same towns, parts of towns, and even houses, which had been formerly affected by it, affords additional proof, if it be necessary, that local circumstances have at least great influence in determining its seat. For example, the earliest case of cholera in Chelsea in 1848 is said* to have been in White Hart-court, and there it continued to exist until the end of the epidemic in 1849. The first case in 1854 was in the same place, perhaps also in the same house, for deaths occurred in the same house in both visitations. A very similar fact is presented by Augusta-court,* in which the three earliest fatal cases of cholera in Chelsea occurred in February 1832, and which being revisited in 1854, continued to furnish victims to the pestilence throughout the entire duration of the outbreak. Kent-street† and Mint-street, Southwark, which were severely visited at an early period of the last epidemic, were also amongst the first seats of cholera in 1832. We also learn from Dr. Acland's very valuable 'Memoir on the Cholera in Oxford in 1854,' that, with one exception, every yard and street in St Thomas's parish, which had been attacked by cholera in 1832 and 1849, was revisited in 1854. (p. 39.) Thus, whatever other conditions may be necessary to the development of cholera, it is evident that some local circumstance plays a very important part in its evolution. Into the nature of this local element we now propose to inquire, with the aid of the works before us, using at the same time such facts as have fallen under our own observation. As our purpose, however, is not to suggest a theory of the causation of cholera, and then to select only those facts which seem to afford it countenance, we must prepare the way for this investigation by first of all referring briefly to the chief theories that have been suggested to explain the operation of local conditions in cholera; and secondly, by examining the several circumstances which have been set down as "its determining local conditions."

The existence of local causes of insalubrity is almost universally considered necessary for the evolution of a cholera-epidemic, although very great diversity of opinion exists as to the part they bear in the production of the pestilence. By most persons, the unwholesome conditions to which the dwellers in unhealthy districts are habitually exposed, are believed to produce a low tone of the general health, and proclivity to disease, which disable them from resisting the exciting cause of the epidemic. Dr. Carpenter‡ surmises that these influences, and also other causes of a more personal nature, produce a condition of the blood itself which predisposes it for zymotic action, the precise character of which depends upon the nature of the exciting cause with which it is brought into relation,—the special poisons of small-pox, scarlatina, typhus, or cholera, for example, being each capable of exciting

* Cholera Gazette (published by authority of the Central Board of Health), p. 205. London, 1832.

† Loc. cit., pp. 121, 156, 218.

‡ British and Foreign Medico-Chirurgical Review, vol. xi. pp. 161–177.

its peculiar fermentation in blood already charged with organic compounds in a state of retrogressive change. Without entering upon the general question so ably argued by Dr. Carpenter, we may be permitted to doubt whether cholera be a zymotic disease in the sense here intended, whether the action of its exciting cause be not rather simply toxical, and, if fermentation has any share in its production, whether this does not occur externally to the organism, and produce rather the exciting cause than the disease itself. Moreover, that persons arriving from a pure atmosphere, and in sound health, have so frequently shown themselves peculiarly prone to suffer from a brief exposure to the epidemic influence, is altogether at variance with the opinion that the supposed condition of the blood is a necessary predisposition for cholera.

The opinions propounded by Drs. Barton and Pettenkofer, whilst they differ materially from each other, are just the converse of those already referred to, since they seem to infer a simply toxical action on the part of the poison of cholera. Dr. Barton considers epidemic pestilences as the direct consequences of the co operation of certain meteorological conditions with a local cause. This local cause he believes to be "filth, moisture, and stagnant air," and especially the emanations arising from extensive upturnings and exposure of a soil impregnated with the results of organic decomposition. The efficacy of such disturbances of the soil in the production of outbreaks of epidemic disease, is illustrated by a chart exhibiting the mortality per thousand of New Orleans for each year from 1787 to 1854, together with the presumed cause of the excessive mortality which occurred in several of these years.* This very remarkable and valuable document clearly shows how a large mortality, and especially a prevalence of epidemic pestilence, has uniformly accompanied any extensive disturbances of the soil for the construction of canals, pavements, or other public works. In 1832, the most fatal year of the series—when the deaths from cholera amounted to 78·78 in each 1000 persons living, and the gross mortality to 147 in 1000, or upwards of one-seventh of the entire population—there had been extensive digging for the foundation of a street in the preceding autumn, followed in the year itself by similar diggings for the formation of a canal and pavements, regardless of the season. In the latter part of October, 1848, two canals were cleaned out, and two acres of ground were excavated, with the removal and exposure of upwards of 336,000 cubic feet of earth, for the foundation of the new Custom House in the heart of the city. This work lasted until August, 1849, and during the period of the exposure of this mass of soil, saturated with the impurities of the swampy city, there was a severe epidemic of cholera, with a mortality of 3600. In fact, in every year in which an epidemic outbreak, whether of yellow fever or cholera, has occurred, a similar exposure of the soil had previously taken place, the precise form of epidemic being, in Dr. Barton's opinion, determined by the meteorological phenomena of the season. Thus, Dr. Barton evidently considers cholera to be caused by a poisonous miasm, and believes this miasm to be altogether of indigenous origin. Dr. Pettenkofer, on the other hand, believes that the introduction of a ferment from without is necessary for the production of cholera, but thinks that

* Report on the Sanitary Condition of New Orleans, pp. 280, 447, 461.

this ferment can only act where it meets with suitable local conditions. Whilst Dr. Carpenter believes that the foulness of localities "taints the blood of persons exposed to inhale their emanations, and thus produces in them a personal predisposition for zymotic disease, Dr. Pettenkofer is of opinion that the special "leaven" sets up a zymosis, or series of decompositions, in the impure soil itself, and that the special poison of cholera is a miasm generated by this earthy fermentation. Whilst he considers the presence of a special ferment as essential to the production of a cholera-epidemic, he also insists upon the existence of certain local peculiarities. These consist in a damp subsoil, sufficiently porous to be penetrable by the decomposition products of human and animal excrement. It is only in such soil, thoroughly impregnated with this peculiar organic matter, that the special cholera poison is generated. Hence Dr. Pettenkofer says the susceptibility or insusceptibility of towns for a cholera-epidemic is in exact proportion to their "soil relations." The difference between the mortality from cholera in the upper and lower terraces of London is hence attributable to the dry gravelly soil of the former, which naturally allowed all the matters for decomposition to gravitate towards the moist closer soil of the lower levels, where it underwent a much slower decomposition. Entertaining the belief that cholera has never prevailed epidemically upon rock, Dr. Pettenkofer readily accounts for the supposed fact on the ground that the excrement cannot penetrate into the soil, and that the rock neither takes up nor gives off moisture. Single cases may, he says, occur in towns or houses whose foundation is rock, but epidemics never; referring to some alleged cases which seem opposed to this opinion, he says the exceptions are more apparent than real. In this respect, however, Dr. Pettenkofer is mistaken, for cholera has occurred sufficiently often on rock to prove that at least the porous soil to which he attaches so much consequence, is not a necessary element in the production of cholera. Arguing from this presumed fact, Dr. Pettenkofer confidently asserts that we must abandon all idea of the air and water as the nidus of cholera, and seek for it in the soil alone.*

The ferment supposed by Pettenkofer to be necessary to set up the peculiar decomposition of which the cholera poison forms one of the products, is the matter of the dejections of cholera patients. His notion is, that the cholera-germ-bearing excrement which spreads itself in the damp porous soil, already impregnated with fæcal matters, produces, by means of the fine division which it there undergoes, such a modification in the process of putrefaction and decomposition, that, in addition to the gases usually formed, a cholera miasm is produced which becomes diffused through the atmosphere of dwellings, in common with other exhalations. Thus, although the cholera miasm is formed in the ground, the air is the vehicle for its transmission to the patient. Dr. Pettenkofer adduces several instances in which he supposes cholera to have been imported by means of the dejections of persons suffering either from diarrhœa, cholerine, or cholera, for he views these diseases as mere varieties, and infers that if the dejections of cholera patients be capable of originating the pestilence, those of persons suffering from either of the milder complaints most probably produce the like result. The most circumstantial account

* Pettenkofer, pp. 104, 110, 37, 38.

of the introduction of cholera by the supposed means given by Dr. Pettenkofer, refers to the convent prison of Ebrach. Here both the male and female prisoners were attacked, whilst the officials, a company of soldiers quartered there as a guard, and the inhabitants of the adjoining village, entirely escaped. On inquiry, it was found that the first person attacked by cholera was a prisoner who, passing through Munich on his route to Ebrach, was lodged in the prison there, among the inmates of which there had already been several cases of cholera. Arriving at Ebrach on August 20th, he was placed in separate confinement with three other prisoners, and supplied with prison clothes. In a few days after his arrival he reported himself sick with diarrhœa, from which he had also suffered at the period of his departure from Munich, was admitted into hospital, and suffered an attack of cholera, from which he rapidly and completely recovered. On August 27th, the person who attended him during his illness, took cholera and died, and in a few days the epidemic extended throughout both the male and female divisions, between which there had been no intercommunication excepting through the officials and the guard, all of whom, as already related, continued healthy during the entire course of the epidemic. The first case among the female prisoners was that of a woman who had washed the linen of the male prisoner on the day after his arrival from Munich, before cholera had developed itself in him, and several days before he reported himself sick. This woman passed through the disease in the milder form of cholerine, and, like the male prisoner, rapidly recovered. None of the three prisoners with whom the man was confined prior to his illness were attacked. There were in the prison six hundred male prisoners, arranged in classes, between which there is little communication, yet the disease showed itself speedily throughout all parts of the prison, reached its climax in the men's division on September 11th, in the female division on September 13th, and then declined, having carried off about ten per cent. of the prisoners. From this history it is inferred that the disease had been introduced by the prisoner from Munich, and from him been disseminated throughout the establishment. Dr. Pettenkofer, however, says that the disease was not propagated by contagion, no clue to its spread by means of personal intercourse having been elicited by the most careful inquiry. The three prisoners with whom the first patient was originally confined could not aid in the propagation of the disease, both because none of them personally suffered from the epidemic, and because they were not liberated from their isolated confinement so as to mix with the other prisoners until after the disease had become general. The large cesspools in the garden, into which the stools of the already infected prisoner from Munich had passed, and the badly-arranged necessaries of the women's division, into which all their dejections were emptied, are considered by Dr. Pettenkofer as having formed the centres of infection, from which, by means of the fermenting process set up in the excreta brought to these points, the exciting poison of the disease was distributed throughout all classes of prisoners.* Elsewhere, Dr. Pettenkofer says that the most intimate communication between places may occur, without leading to the introduction of cholera; while, on the contrary, this disease has often

* Loc. cit., pp. 123 et seq.

broken out in places whose communication with the cholera sick could not be demonstrated. The former fact he explains on the supposition that the requisite soil-relations were wanting. In Munich, the outbreak of cholera was preceded for a full month by a general prevalence of diarrhœa and summer cholera of such a character that, had epidemic cholera been present, they would have been attributed to the epidemic cause, and been viewed as mild cases of cholera. Out of five hundred officials employed in the Industrial Exhibition, very few escaped this simple disease; and although it would seem that all recovered, since the first death recorded as occasioned by cholera took place on July 29th, some of them were so severely prostrated by the disorder as to be with difficulty removed home. In the three first cases of developed cholera in Munich, no intercourse with cholera patients, or with persons who came out of neighbourhoods in which the epidemic was raging, could be demonstrated, neither was there any ground for supposing this to have been the case. Do not these facts, like similar ones in this country formerly referred to,* point rather to the spontaneous production than the extrinsic origin of cholera?

Dr. Pettenkofer is well read in the writings of English authors on cholera, and refers to them on several occasions in his work, which forms a portion of the Report of a Commission appointed by the Minister of the Interior to conduct scientific investigations into the Indian cholera. We cannot help surmising that he has, perhaps unintentionally, borrowed his idea of the agency of the cholera dejections in the production of cholera from our fellow-countryman, Dr. Snow; but that, not finding the Doctor's views to accord with the history of cholera, he has, in common with Thiersch—who also attributes the propagation of cholera to the rice-water stools of cholera patients, in a state of fermentation—essentially modified the original suggestion. Although we disbelieve Dr. Snow's theory, we are firmly of opinion that to him of right belongs all the credit that may attach to the suggestion, that the evacuations of cholera patients are either directly or indirectly the means of spreading this disease.

The several opinions we have cited, however much they differ in other respects, agree in considering some local condition or other as necessary for the production or development of cholera, save only that Dr. Carpenter believes the predisposition to zymotic disease—and he considers cholera as a zymotic disease—may be induced by personal as well as by local causes. Dr. Pettenkofer's view of the nature of the local causes of cholera is sufficiently definite and simple, and to it we shall have no further necessity to refer. With these exceptions, nothing can well be more vague and unsatisfactory than the opinions that have been usually expressed as to the nature of the localizing causes of cholera. Unmindful of the proposition, that every effect must spring from some definite cause, it has been common with sanitary inquirers at once to refer the same effect to several causes, and several effects to the same cause, instead of endeavouring to trace each result to its proper origin. Thus it has frequently been said, that cholera and fever arise, or are localized, by the same causes, run in the same track, and haunt the same localities.† First, we believe,

* British and Foreign Medico-Chirurgical Review, vol. xvii. pp. 289, 291.

† Report of the General Board of Health on the Epidemic Cholera of 1848–9, pp. 20–22.

promulgated in this country, this assertion has been repeated by Pettenkofer and other foreign authorities. No doubt fever and cholera do very often visit the same localities, and prevail amongst the same classes of persons; but this arises from the co-existence in such cases of the conditions incidental to both diseases, and not from both being the normal consequence of the same conditions. There are many localities in which fever is rife, that cholera has passed over. There are others in which cholera has been prevalent, but fever a rare visitant. It would, indeed, be easy to point to others which have been severely visited by both; but it is almost unnecessary to observe, that this fact points to no necessary connexion in the ætiology of the two diseases. These assertions, the result of personal observation, are strikingly confirmed by Mr. Simon, who thus expresses the result of his very wide experience during the cholera epidemic of 1849, in his Second Annual Report:

"On the one hand, it is unquestionably true that many habitual seats of fever were visited by cholera; on the other hand, many of the worst fever nests in the whole metropolis were unaffected by it; and it struck with extreme severity in a class of houses habitually exempt from fever. See, for instance, how malignantly it prevailed along the line of Farringdon and New Bridge-streets, and in Fleet-street and Ludgate-hill, where their line intersects that just mentioned; and here, you will observe, not only in those obscure and ill-ventilated courts and by-ways where fever is the familiar visitant of a hungry and crowded population; but also, and very strikingly, in spacious and airy houses situate along the main thoroughfare of the City, and inhabited by opulent tradesmen, by members of the various professions, or by officers of assurance companies." (p. 94.)

Neither is it true, as has frequently been affirmed, that cholera has almost exclusively visited such places as are liable to frequently-recurring attacks of other epidemic or zymotic diseases, and the death-rate whereof is high. A very trustworthy and remarkable example of the contrary is afforded by the "cholera area" of St. James's, Westminster, the particulars of the outbreak in which are so admirably described in their Report by the committee nominated by the vestry to investigate the history of that sad visitation. This district, although one of the most densely peopled in London, has been found, on a careful inquiry into its mortuary statistics for the preceding seven years, exclusive of the few days of epidemic visitation in 1854, to have sustained an annual mortality of only twenty and a half in the thousand, of which less than one twenty-second part was occasioned by zymotic disease.* "It likewise deserves mention, that, of the 537 cholera deaths of the late epidemic, 323 occurred in houses which, during the past seven years, had suffered no deaths from other zymotic disease."

Amongst the local conditions that have been supposed, almost in an equal degree, to develope an outbreak of cholera, poverty; the over-crowding of houses; defective house ventilation; want of cleanliness; dampness; impure water; lowness of site; the effluvia from the decomposition of the various organic *débris* allowed to collect in poor and neglected localities; the emanations from human and animal excrement, whether accumulated into cesspools, or allowed to rot in foul drains or lay-stalls; the malaria from fetid ditches; the miasms from City grave-yards; and the stinks from knackers' yards, bone-crushing, catgut-spinning, and

* Report of Committee for Scientific Inquiries, p. 50.

other offensive establishments, have been insisted on by different authorities.* That each and several of these circumstances have been found associated with cholera is an unquestionable fact. That most of them are by no means its necessary adjuncts is not less true. Whilst cholera has not spared the thinly-occupied houses of wealthy persons, it has very often left harmless some of the most densely-crowded cottages of the poor; and whilst it has gathered victims from the main streets, it has occasionally left untouched the courts and back slums in which offensive accumulations are allowed to form, and offensive trades are wont to be carried on.† Witness, for example, the prevalence of cholera in 1849 amongst the families of the respectable and wealthy tradesmen, professional men, and others, mentioned by Mr. Simon as occupying houses in Ludgate-hill, Farringdon-street, and the neighbourhood. It would be easy, did our space allow, to cite many illustrations of the same fact that have fallen under our own observation, as well as of its opposite; of localities notorious for the co-existence of several of these insalubrious agencies, which have remained unvisited at times when the epidemic was raging in their neighbourhood. We may therefore set aside most of the presumed local causes as in no degree necessary to the development of cholera, and concentrate our attention upon impure water, lowness of site, and the emanations arising from the decomposition of animal refuse.

That impure water has a powerful influence over the intensity of cholera outbreaks is unquestionable. In his Report to the General Board of Health on the cholera epidemic of 1849, Dr. Sutherland says, that the injurious effect of unwholesome water had been manifest in nearly every affected place; and adds, that "a number of most severe and fatal outbursts of cholera were referrible to no other cause except the state of the water supply," and this especially where "the water was obtained from wells into which the contents of sewers, privies, or the drainage of graveyards had escaped."‡ Since that time much additional evidence of a confirmatory character has been collected. Two examples are recorded by Dr. Acland, in his valuable and interesting 'Memoir on the Cholera in Oxford;'§ the parish of St. Clement's, which suffered a large mortality in 1832, when the inhabitants had filthy water from a sewer-receiving stream; and an insignificant mortality in 1849 and 1854, when the water was derived from a purer source. The other case is that of the county gaol, in which cases have occurred in every epidemic; whilst the city gaol, which is not far from the other, has uniformly

* See Report on the Cause and Mode of Diffusion of Cholera. By W. Baly, M.D., F.R.S., &c. drawn up by desire of the Royal College of Physicians, pp. 16–20.

Report of the General Board of Health on the Epidemic Cholera of 1848–9, pp. 36–60; also Dr. Sutherland's and Mr. Grainger's Reports, forming Appendices A and B to the above.

Registrar-General's Report on Cholera in England in 1848–9, pp. 57–70.

Sanitary Condition of the City of London. By J. Simon, F.R.S., &c., p. 234.

† Green-street, Southwark, contained, at the time of the cholera epidemic of 1854, one knacker's yard, two bone-boiling and crushing establishments, besides a large catgut factory, the combined smell from the whole being most sickening, yet only one death from cholera occurred among its inhabitants, and no unusual number in the streets immediately around it. Compare this with Suffolk-street, also in Southwark, where deaths occurred in the houses of twenty respectable tradesmen.

‡ See Appendix A to the Board of Health Report on the Epidemic Cholera of 1848–49, pp. 14–16.

§ Dr. Acland's Memoir, pp. 51–52.

escaped. The only apparent difference between the two establishments in 1854, seems to have been that the supply of water for the use of the county gaol, and of which the soup and gruel were made, was pumped from a filthy mill-pool within ten feet of one of the prison drains. No sooner were the supply-pipes disconnected with this impure source, than cholera and diarrhœa ceased. Inquiries into the effect of the water-supply of the southern districts of the metropolis over the cholera epidemic of 1854, were instituted by Dr. Snow and the Registrar-General. These inquiries were, however, necessarily imperfect and inconclusive, for the local registrars were unable to return the source of the water supply in 803 out of 4059 fatal cases in houses supplied by the Lambeth and Southwark and Vauxhall Companies;* and Dr. Snow's laborious inquiry, which was limited to the early period of the epidemic, ceased before the disease had reached its height. A yet more elaborate and perfect inquiry was made by the General Board of Health at the close of the visitation, the results of which are given in the Report of the Medical Officer of the Board. These results are especially valuable, because they refer to two large sections of the population, residing in the same localities, "breathing the same atmosphere, comprehending the same classes, and averaging the same habits of life;" in short, placed in circumstances nearly identical, save that the one section, comprising a population of about 268,171 persons, drank impure water; whilst the other, numbering about 166,906 persons, used a clearer and comparatively pure water. The mortality from cholera among the drinkers of impure water—of water impregnated with the sewage of the metropolis, and containing in solution a large quantity of saline matter, derived from the intermixture of sea-water†—being at the rate of 130 to every 10,000, that of the drinkers of the purer water being only at the rate of 37 to every 10,000 persons living.‡ This evidence is greatly strengthened by being placed side by side with the mortuary statistics of the epidemic of 1848–9 in the same district, and by a comparison of the nature of the water supply on both occasions. The Lambeth Company, which in 1854 gave the pure water, supplied in 1848–9 even a worse water than the Southwark and Vauxhall Company. From the figures already quoted we learn that the population to which the Lambeth water was distributed in 1853-4, suffered a mortality less than one-third of that sustained by the drinkers of the water purveyed by the Southwark and Vauxhall Company. From a comparison of the mortality in the two epidemics, it appears that the tenantry using the purer water supplied by the Lambeth Company in 1853–4, suffered not a third as much as the same tenantry had done in 1848–9, when the water was impure. "On the other hand, the Southwark and Vauxhall Company, which pumped an impure water in 1848–9, pumped even a worse water in 1853–4;" and in consequence, notwithstanding "the general metropolitan pressure of the epidemic in 1853–4 was considerably lighter than in 1848–9, the houses supplied by the Southwark and Vauxhall Company in the late epidemic

* Weekly Return of Births and Deaths, vol. xv. p. 515.

† See Dr. Dundas Thompson's Report on the Chemical Composition of Metropolitan Waters during the Year 1854, in Appendix to Board of Health Report.

‡ Report on Impure Water, pp. 6–9.

suffered probably ten per cent. higher mortality than the same houses in 1848–9;" "the comparison of the two populations in the two epidemics stands thus:—In the one population (notwithstanding a generally lighter invasion of the disease) the cholera death-rate rose from 118 to 130; in the other it fell from 125 to 37.

But large as is its influence over cholera epidemics, impure water is not a necessary factor of cholera. The drinkers of the purer and comparatively uncontaminated water of the Lambeth Company in 1854 sustained a considerable mortality; and cholera has often prevailed with great malignancy in districts where the drinking water was perfectly free from fæcal contamination, as in the parish of Tynemouth, in 1849, and in several places named in Dr. Baly's Report.* It follows, therefore, that impure water is either only an accidental and occasional vehicle for conveying the poison of cholera into the system; or that, just as unwholesome food, or the injudicious use of purgatives are determining or aggravating causes of cholera during an epidemic visitation, so is water impregnated with organic impurities. Probably, impure water acts chiefly, if not exclusively, by aggravating individual cases of the pestilence, causing such as might otherwise have been cases of simple diarrhœa, to pass rapidly into the state of "collapse." A careful consideration of the history of the sudden and severe outbreak in the Golden-square and Berwick-street districts of St. James's, Westminster, in 1854—which was apparently connected with the dietetic use of water from the Broad-street pump, found at a later period to have been vitiated by the leakage from a cesspool—appears to support this supposition. It should, however, be remembered when studying this visitation, that it occurred just when the pestilence was at its height. A similar, though less violent, outbreak occurred simultaneously at Rotherhithe; and several smaller districts which had previously escaped, also suffered at that period; thus showing that the epidemic influence was at that time most general—perhaps, also, most intense. Of the earlier cases in this memorable outbreak, it is reported "that premonitory diarrhœa was of short duration, or altogether absent." It is also said that no "certain information can be collected as to the relative amount of diarrhœa." Judging from the tables published in the Appendix to the Board of Health Report, the cases of diarrhœa in this district were fewer than those of confirmed cholera; whereas, according to the calculations made by the Medical Council of the General Board of Health, it appears that, in the metropolis at large, 1310 persons out of every 10,000 were, on the average, attacked by diarrhœa of some severity, whilst 99 only suffered from cholera. This is exclusive of milder diarrhœa, from which it is computed that 2064 in every 10,000 persons living suffered.† An examination of the mortuary statistics‡ gives additional probability to our supposition, for whilst the comparative mortality of cholera and diarrhœa in London, during this epidemic, is found to have been 46 by cholera to 25 by diarrhœa, the deaths in the Berwick-street and Golden-square districts were 477 from cholera to 37 from diarrhœa.§

* Dr Baly's Report, pp. 201–5. Liverpool and Edinburgh also afford good illustrations of severe outbreaks of cholera in towns where it seems impossible for the water to have had any influence.

† Report of the Committee for Scientific Inquiries, p. 10. ‡ Loc. cit., pp 10, 98.

§ The disparity was even larger than is here represented, for if the deaths of persons who,

This discrepancy becomes still more evident when the mortality occasioned by these diseases in the two already-named districts of St. James's, is compared with that in the bordering registration districts. Thus the mortality from cholera in the five sub-registration districts of Hanover-square, Charing-cross, Long-acre, All Souls, and St. Ann, amounted to 303, that from diarrhœa to 151—numbers which pretty nearly accord with the general metropolitan average, and are quite disproportionate to the comparative mortality from the same diseases within the "cholera area" of St. James's, which they surround on all sides.

It was announced by Dr. Farr, in his Report on the Cholera in England in 1848–49, that, as regarded London, "the elevation of the soil has a more constant relation with the mortality from cholera than any other known element; the mortality from cholera is in the inverse ratio of the elevation."* This statement was fully borne out, so far as London is concerned, by the history of that epidemic, and is in the main confirmed by the experience of the late visitation, although the mortality was then found not to be "so invariably in each district inversely as the elevation," as on the previous occasion. This general rule, "that the mortality of cholera is inversely as the elevation of the people assailed above the sea level," is frequently quoted without reference to the explanation of the circumstance offered by Dr. Farr, which does not differ essentially from the opinion of Pettenkofer, that cholera prevails more intensely in the low districts, because all the organic impurities of the higher ground, gravitating thitherward, there undergo chemical action. Although, then, the experience of London, and of many places in England, in 1849, was such as fully to justify the assertion, Dr. Farr elsewhere says, "that cholera will not only be fatal on low ground, but on high ground, if, from any concurrence of circumstances, the conditions exist there which are so constantly found in alluvial soils, lying on a level with, or below, the tidal waters."† Thus, cholera was more fatal in the village of Wrekenton, situated 500 feet above the river Tyne, in 1849, than in the narrow low-lying lanes and alleys of Gateshead which border the river's margin. Out of a population of 700, 100 died in the course of fifteen days.‡ With a single exception, the mortality in New York from cholera, in 1849-50, was greatest in the sixteenth ward, where it produced 778 deaths:§

"And when the reader is informed that this ward mostly occupies very high ground—that it is neither thickly covered with buildings, nor densely populated, he will at once begin to conclude, as others have before him, that it militates strongly against the idea that lowness and dampness favour the prevalence of the cholera. And perhaps no more striking illustration of the necessity of a full and minute knowledge of all the facts, and the danger of judging from a few, could be adduced than this."

taking the disease within the district, died beyond its limits be added to the above, the total deaths, as calculated by the parochial committee, amounted to nearly 700. See Report of the Cholera Inquiry Committee, p 15.

* Report on Cholera in England, in 1848–9, p. lxi. † Loc. cit., p. lxx.

‡ See Report to the General Board of Health, on the Sanitary Condition of Gateshead. By Robert Rawlinson, Esq., Superintending-Inspector, pp. 63–70. This Report contains very interesting accounts of the outbreak in Wrekenton, by Mr. Davis, surgeon, of that place, and Mr. Bennett, F.R.C.S., of Gateshead.

§ Dr. Wynne's Report, pp. 91–92.

Professor Davis, from whom Dr. Wynne is quoting, goes on to explain that the district, although reputed as the highest, driest, and most airy portion of the city, is undrained by sewers, and badly supplied with water; that the surface is uneven, and closely underlaid with micaceous slate, which frequently crops out; and that there are pools of stagnant water, which remain until evaporated by the sun. Hull excepted, Merthyr Tydfil, in Glamorganshire, including the outlying village of Dowlais, suffered more severely from cholera in 1849 than any district in the kingdom. The mortality from cholera and diarrhœa conjointly amounted to 1779 out of a population not exceeding 53,000. Merthyr is 500 feet, Dowlais 1000 feet above the level of the sea.* Abroad, cholera has been most fatal at much higher elevations, as at Bogota, 9000 feet above the sea level and several hundred miles from the coast; and at Mexico also, 7100 feet above the ocean. In Jamaica, it was fatal, in Newcastle, Manchester, Moneague, and other places elevated from 2000 to 3000 feet above the level of the sea; thus showing that elevation in itself only influences the disease so far as it affects the local conditions, and verifying the already-quoted opinion of Dr. Farr. The Report of the Registrar-General on the cholera epidemic of 1848 and 1849, is exclusively based upon the mortuary records. At that time, no reliable means existed whereby the number and allocation of cases could be computed. The committee of the Medical Council for Scientific Inquiries endeavoured to procure statistics of the number of cases of cholera and diarrhœa during the visitation of 1854; although, from the late period of the epidemic at which the inquiry commenced, the facts brought before them are incomplete, the very important conclusion has been deduced,—"that the *cholera-leaven*, be it what it may, was scarcely less diffused in the districts that suffered the lowest mortality, than it was in the districts where the disease was tenfold more fatal." We must refer to the Report of the committee for the very interesting facts and reasonings upon which this conclusion is grounded, as we have not space for a full quotation, and the facts are so tersely stated as to be incapable of condensation. It appears clearly, however, that if the mortality per-centage of cases of cholera and diarrhœa was the same in the higher terraces of the metropolis as in the lowest, the comparative number of persons attacked either by cholera or diarrhœa of some severity, would be about 1288 in the 10,000 in the higher regions, and 1741 in the lower regions of London. There are, however, grounds for supposing that whilst the proportional mortality from both diseases was higher than the average in the low-lying districts, it was much below the average in the higher districts, and that thus the "proportional number of persons that were attacked by diarrhœa or cholera in each must have been about 1490 in a myriad of the population."†

Having thus found that several of the presumed localizing causes of cholera are at least not necessary to its existence, since none of them has been found on all occasions to co-exist with cholera, which, on the other

* Registrar-General's Report on Cholera, p. xxxii. Report to the General Board of Health on the Sanitary State of Merthyr Tydfil. By T. W. Rammell, Esq., Superintending-Inspector, p. 7, 1850.

† Report of the Committee for Scientific Inquiries, p. 15.

hand, has sometimes prevailed in their absence, it only remains for us to consider the influence of an atmosphere contaminated by the effluvia arising from decaying organic matter. It has long been known that the alimentary canal is very sensitive to such influence. Dr. Cullen long ago remarked,* that the effluvia from very putrid animal substances readily produce diarrhœa—an observation which has been confirmed by later observers. Most probably both the nature of the decomposing matter, and of the transformative process it is undergoing, are important elements in regard to the effect on the human constitution. Certain at least it is that districts in which the most powerful putrid odours tainted the air, have sometimes almost entirely escaped, whilst others contiguous to them have suffered severely.† We have personally taken much pains to investigate the precise conditions which from their more uniform co-existence with it, might be supposed to produce or to aggravate epidemics of cholera. The result of our observations has been that an atmosphere impregnated with the products of fermenting excrement is at once the most obvious and most constant concomitant of cholera. These exhalations were often found, even in a concentrated form, in houses where the existence of any palpable cause of insalubrity would scarcely be suspected, and thus the fact that the pestilence sometimes passing over slums and rookeries, knocked at the door of the comfortable annuitant or the wealthy tradesman, is readily explicable. During the epidemic of 1854, in a portion of the parishes of Chelsea, and of St. Saviour's, and St. George's, Southwark, exclusive of cases in which the notes made at the time of inspection are not explicit, we personally examined into the sanitary state of the houses occupied by 392 families, in whom deaths from cholera had occurred. Out of 2701 persons, 616 had cholera, besides 871 cases of diarrhœa. Four hundred and fifty of the cholera cases proved fatal. The inquiry extended alike to the dwellings of wealthy residents in good streets, as to those of lodgers in the most overcrowded and filthiest alley. Without devoting more space to the subject than we can afford, it would be impossible to convey the evidence in its fulness as it came before us, and tested as it was in every manner that we could think of, but an analysis of the numbers above given shows that the existence of the products of the peculiar decomposition alluded to were evident to the senses at the time of visit in 213 houses. In nineteen of these there were cesspools situated either below the house itself, or in such close proximity thereto that the soil had percolated through into the subsoil below the dwelling. In 220 cases, open privies were either erected against the main wall of the house, or so near to the back entrance as to allow of the emanations from the soil being observable within doors. In seventy-seven instances, branch drains of imperfect construction, having direct communication with the common-sewer, passed underneath houses; or a foul, open ditch; the main-sewer, or a principal branch, in a ruinous condition, was so near to the house as to influence its internal atmosphere.‡ In ninety-two of the houses were

* Cullen's First Lines of the Practice of Physic, by J. Rotheram, M.D., vol. iii. p. 118.

† As in the case of Green and Suffolk-streets, Southwark. See note, p. 70.

‡ For examples of these facts see Dr. Sutherland's Report on Epidemic Cholera in the Metropolis in 1854, pp. 30–34.

untrapped sinks or drains, connected directly with the street sewer, by which the foul sewer exhalations were conveyed to the internal atmosphere. Of the entire number of houses, in only thirty-three were no sources of this atmospheric contamination detected. Not to dilate further upon the precise manner in which the air breathed by the inmates became corrupted with this foul impurity, let it suffice that persons appeared to suffer in proportion to the contamination of the air they breathed by the "privy odour," and that immunity from this appeared to secure immunity from cholera. Strong confirmation of the opinion that the effluvia from decomposing cesspool soil are capable of inducing diarrhœa, is also afforded by a fact which came under our observation in the summer of 1855. Being requested by the General Board of Health to investigate certain alleged outbreaks of choleraic disease in the metropolis, we found that there had been a general outburst of diarrhœa in three or four contiguous streets of Bethnal-green, shortly after the opening of the ground for the construction of sewers, and the consequent disturbance of several cesspools. Whilst the upturned soil was exposed to the air, the atmosphere of the affected streets, never remarkable for its fragrancy, was excessively offensive, and to this cause the prevalence of diarrhœa was attributed, both by the residents and by several official visitors of the district; an opinion much strengthened by the disappearance of the disease as soon as the work was completed, and likewise by entire immunity from similar disease enjoyed by the inhabitants of surrounding streets precisely the same in character to those affected, but in which there had been no disturbance of the soil.

Let us now proceed to compare these observations with those of the several authors whose works are placed at the head of this article. It will be seen that they afford a strong confirmation of their correctness. Mr. Simon, in his Fifth Annual Report to the City Commissioners of Sewers, whilst he appears to consider the introduction of a ferment from without to be requisite for the production of cholera, says—

"The specific migrating power—whatever its nature, has the faculty of infecting districts in a manner detrimental to life, only when their atmosphere is fraught with certain products susceptible, under its influence, of undergoing poisonous transformation. . . . . Through the unpolluted atmosphere of cleanly districts it migrates silently, without a blow: that which it can kindle into poison, lies not there. To the foul, damp breath of low-lying cities, it comes like a spark to powder. Here is contained that which it can swiftly make destructive,—soaked into soil, stagnant in water, griming the pavement, tainting the air—the slow rottenness of unremoved excrement, to which the first contact of this foreign ferment brings the occasion of changing into new and more deadly combinations."*

The similarity of this view to Pettenkofer's, save in the supposition that the cholera dejections constitute the specific ferment, and that the lethal fermentation goes on in the soil, the atmosphere being only secondarily vitiated, cannot fail to occur to all our readers. But, in fact, Pettenkofer affords ample evidence in his Report, of the existence of abundant sources of this atmospheric contamination where cholera prevailed. Thus, after mentioning the inadequate surface drainage of Munich, he states that the

* Sanitary Condition of London, pp. 236, 234.

excrement of the population is collected into brick pits, or removed into cesspools from necessaries situated either within doors or near to the houses. These necessaries communicate with the cesspools by means of untrapped wooden spouts, through which the soil is conveyed, thus allowing a free passage for the gases of decomposition into the interior of houses. In other cases, the only conveniences are night-chairs, the contents of which are emptied into a cesspool, one of which often serves for several houses. The cesspools and pits are most commonly not water-tight, so that the loose earth below is completely penetrated by the liquid portions which there undergo slow decomposition. These cesspools and pits are periodically, but not frequently, cleansed. Pettenkofer estimates the daily production of excrement in Munich at 300,000 pounds, of which, as not more than a tenth part is removed, nine-tenths must putrify immediately around the dwellings of the inhabitants.* The "necessary" accommodations of the prison in the old Convent of Ebrach are described as consisting chiefly of wooden night-chairs. Such privies as exist for the use of the prisoners, empty themselves into a stream, which, entering at the women's division, runs through the institution, and passes out of it at the men's side. The privies in the female division are thoroughly bad; the brickwork through all the floors is impregnated with excrement, which has even coloured the external surface brown. "The stink is a pestilential one." The privy-doors on all the stories are close to the entrances of working or sleeping-rooms, whilst the doors of rooms on the opposite side of the female division are similarly situated with regard to the wooden spouts by which the soil is conducted from the necessaries to the ditch. Dr. Pettenkofer elsewhere says, that the effluvia from the excreta entered the prisoners' sleeping-rooms; and, indeed, lays the greatest weight upon its doing so whilst the prisoners were asleep, this being, he says, the period when the organism is least able to resist the poison. At Gaimersheim, a village containing 974 inhabitants, and which enjoys a melancholy celebrity from having been nearly depopulated in times of plague, the ravages of cholera were confined to certain houses, whilst other groups of houses altogether escaped. The population of the houses attacked was 291, of whom 111 were seized by the pestilence, and 80 died. Water, it appears, is so scarce, that brown cesspool liquid is carefully collected, a pool filled with it being preserved so as to be at hand to extinguish fires. Before passing on to other reporters, we must quote another example of the kind of place in which Dr. Pettenkofer found cholera to prevail. It is Traunstein, where the epidemic confined its onslaught to the inmates of nineteen houses, all of which were carefully examined by the Doctor himself. Most of them were in a low-lying, damp portion of the town, and several of them were both damp and so placed that the moisture and impurity with which the subsoil was charged, must gravitate towards them. The necessaries and cesspools were within doors in thirteen of the houses, and although described as in good condition, were unfurnished with stink-traps. In five others, these conveniences were in walled courts close to the houses; in one only in the open air, and entirely separate from the dwelling. In one house only do the necessaries which are within-doors run into a canal, the fall of which is, however, so trifling, that the soil scarcely runs off, unless when the water is unusually high. The

houses most severely visited by cholera were, without exception, those in which the cesspools are within-doors.

The "terrene cause," to which Dr. Barton attributes the pestilential epidemics of New Orleans, does not differ materially from the "soil relations" of Dr. Pettenkofer. Dr. Barton, indeed, does not limit the local cause to the single source of impurity, to which we, in common with Dr. Pettenkofer, are disposed to attach so much consequence; but his account of the state of the subsoil of New Orleans, whilst it shows the inhabitants of that city to be the victims of all the evils incidental to a residence in an imperfectly cleansed and undrained town, entirely accords with the other facts we have cited. After saying that the most fruitful cause of bad air is night-soil and town refuse, the Doctor points to the peculiarities of New Orleans, by which those evils common to it and all large cities are greatly aggravated. It is impossible to dig pits of two feet in depth, and in summer, in consequence of the rains, of even a less depth, without coming to water. Cesspools, or pits for the reception of night-soil, are therefore not applicable to the circumstances of New Orleans; and as manure is not required in the cultivation of the land, consisting as it does of fine rich alluvial soil, all the excreta of the population—the annual amount of which is estimated by Dr. Barton at five thousand six hundred tons of night-soil, and about fifty thousand tons of urine—are exposed to undergo decomposition within the city itself, exhaling, he says, "their noxious and poisonous gases to the atmosphere we breathe, absorbed by the water we drink, and contaminating our most private recesses, where, the air being mostly stagnant, it is apt to remain permanently."*

After reading Dr. Barton's account of the filthy state of New Orleans, it is easy to understand why the "upturnings" of so polluted a subsoil have always been precursors to an outbreak of pestilential disease. We cease to wonder, either at the frequent and terrible visitations of pestilence to which the inhabitants of this unhappy city have been exposed, or that the death-rates—which, on a sixty years' average, have exceeded 59 in the 1000—should, in sickly seasons, have attained to 89, 102, and even to 141 in each 1000 living inhabitants. It is not unworthy of note that upturnings of the subsoil during seasons of epidemic visitation have seemed to be attended with injurious results in this country. Cholera was peculiarly rife in several parts of this metropolis in 1854, where the ground was being excavated for sewers, and in several districts both the local authorities and the public blamed the works then in progress for severe local outbreaks.

Dr. Milroy's Report affords ample information on the nature of the localities in which cholera prevailed in Jamaica. The dwellings of the negroes, from which the fresh air was most carefully excluded, are described as wretched sheds, destitute of the most ordinary conveniences, and receptacles of the most disgusting filth. The medical men repeatedly speak in their reports of the virulence of the disease being mainly due to the condition of the patients' dwellings, and of the violence of the attacks being proportioned to the greater or less impurity of the

* Dr. Barton's Report, pp. 357–8.

atmosphere within the cabins. Dr. Wingate Johnson, Deputy-Inspector of Fleets, thus describes the parts of Port Royal in which cholera was most fatal, in the Report of the Jamaica Central Board of Health:

"Most of the houses have small courtyards attached to them, which are generally the abode of pigs and goats, and are also invariably the depositories of every species of disgusting filth, such as human ordure, as well as other excrementitious matters, stinking fish-guts, and putrid slops; in fact, everything is there to be found, excepting cleanliness or pure water. The stench perceivable in the vicinity of some of these localities is at times intolerable. The few inhabitants that do observe anything like decency (there being no public privies), generally resort to the beach facing the sea in front of the battery in the vicinity of the church. About this spot the night-soil is also generally deposited. When the sea breeze blows home, this place is directly to windward of the town;" . . . . (p. 35.)

That the disease really found the condition necessary for its development, if not for its actual production, in the impurity of the localities here described, is rendered more evident by the contrast between its ravages among the miserable occupants of such hovels, and the absolute immunity enjoyed by the inhabitants of more healthful dwellings.*

In Kingston, where one-eighth of the entire population was swept off by the pestilence, the amount of contamination upon the surface at all times is described as incredible. The back courts and privies—where there are any—are represented as universally foul, unventilated, and offensive. Speaking of Montego Bay, Dr. Milroy says:

"A large number of the dwellings of the lower classes have no privy accommodation at all. . . . . The offensiveness of the necessaries in many of the larger houses may be judged of from the circumstance that I had been advised not to put up at two of the chief lodging-houses in the town, in consequence of the notorious nuisances in their back yards. The landladies of both houses died from the epidemic." (p. 58.)

Even the barracks were not free from similar sources of atmospheric impurity, and there was accordingly a considerable mortality amongst the troops, although much less in proportion to their numbers than occurred among the lower classes of the civil population. The strength of the military force in the island in 1850 and 1851 was 1770, of whom 756 were white, and 1014 black troops. Cholera was fatal to 29 of the former, and 99 of the latter.

"The state of the privies in all the barracks which are not immediately close to the sea, is altogether most disgusting. It is difficult to exaggerate their abominable condition at Kingston, Spanish Town, and Up Park Camp. . . . . One of the principal thoroughfares in Spanish Town is purposely avoided, in consequence of the horrible pollution of the atmosphere from this cause. Equally bad is the state of things on the west side of Kingston Barracks; where, not to mention the abomination of the large open dung-pit within the walls, sending forth its foul effluvia all round, there are two or three huge vaulted cesspools immediately under the surface, and which, it is believed, contain the accumulated excretions of hundreds of men for a great number of years." (p. 121.)

"So great is the privy nuisance at Up Park Camp, that, in my opinion, it is one of the chief causes of the distressing amount of sickness which has so often, in former seasons, prevailed among the troops there, and of the persistent cleaving of the choleraic poison during last year to this station. The necessaries are three or four in number, situated a little in the rear of the barracks, between them and

* See Dr. Milroy's Report, p. 37.

the hospital buildings. The effluvia from them is at all times most offensive. In the evenings, the land wind brings the tainted atmosphere right upon the barrack-rooms, and the stench is then disgusting. . . . . Nothing can be worse than the construction of these places; they are literally shut boxes over huge pits of ordure, the fluid parts of which soak into the ground, while the solid matter goes on accumulating for one or two years, or longer. . . . . The privies of the officers are in front or to the south of the barracks, which are thus exposed to pollution in both directions." (p. 122.)

In his "general conclusions," Dr. Milroy says that the influence of local agencies over the development and type of the disease was unfailingly shown in every part of the island. "The mortality among the well-conditioned class was very limited; it might be counted by units; whereas that among the mass of the people was by thousands." The atmospheric contamination from filth within and around dwellings was the main exciting and intensifying cause of the disease.

Several of the Indian Reports afford ample evidence that cholera has there been equally partial to localities whose atmosphere was vitiated by the products of fæcal decomposition, as in this country. Jessore, where cholera broke out very malignantly in 1817, is described by Mr. Jameson* as "a crowded, dirty, ill-ventilated town." The pestilence was most prevalent at Calcutta, in districts that were intersected by pools, broad ditches, and channels, from which foul gases were continually evolved. The huts, constructed of straw or mud, "are generally from six to twelve feet square, placed so close to each other as to leave scarcely room to pass between." In these wretched hovels whole families, consisting of six or eight persons, usually reside; "and not unfrequently cows, pigs, and other domestic animals, add to the filth and foul atmosphere in which they abound." Even in the neighbourhood of Government House "there is a stagnant pool in which the whole neighbourhood deposit their filth, and whence a stench of the most noisome and injurious kind frequently proceeds." The same writer describes the cantonment at Meerut, occupied by her Majesty's 14th Regiment in 1819 or 1820, when it lost 41 persons out of 1200 by cholera, as having been very filthy: "The privies were too few in number, and could hardly be kept clean or wholesome. The effluvia proceeding from them was at times exceedingly offensive, even to a great distance; and many of the worst cases were those of men seized in them with violent spasms and vomiting."† Mr. Scot says that the epidemic first broke out at Madras, in Vipery—a situation "abounding with stagnant water, the receptacle for every species of filth"—among the natives residing in some huts about which much offensive and corrupted matter had been accumulated.‡ Nearly twenty years after the publication of Mr. Scot's Report, the medical officer in charge of the troops, in consequence of the repeated outbreaks of cholera in the regiment inhabiting the Vipery lines, advised the thorough purification of the neighbourhood, including the opening and cleansing of the obstructed main drain. These suggestions being adopted, Mr. Rogers, from whom we quote, says it was found, on subsequent inquiry, that the troops inhabiting these lines had escaped on several

* Report on the Epidemic Cholera Morbus in the Presidency of Bengal, in 1817, 1818, and 1819. By James Jameson, Assistant-Surgeon and Secretary to the Board, pp. 107, 110–115.
† Op. cit., p. 312–15.
‡ Report on Epidemic Cholera in the Presidency of St. George, p. 49.

occasions when the disease was raging severely in the neighbourhood.* Mr. Rogers relates a still more apposite fact, with which we must close our illustrations from the Indian Reports:

"The Coom river nearly encircles the village of Chintandrepett. This river was made a privy of by hundreds of natives daily; and when the monsoon was heavy, and the bottom of this Augean stable thoroughly cleansed, no ill effect resulted; but if the monsoon failed, and the river remained uncleansed, when the hot weather returned, the water became low, and the filth at the bottom was exposed to the sun, the smell was most offensive, and an attack of cholera was the certain result, the only victims being those residing within a short distance of its banks."†

In Dr. Wynne's 'Report on Epidemic Cholera in the United States in 1849–50,' are several pertinent facts of a like kind. The history of the outbreak in the Baltimore almshouses, well placed in a healthy locality at a distance from the town, is one of the most remarkable. Out of 632 inmates, including 115 who either eloped or were discharged during the visitation, 99 died of cholera. The account is too long for quotation in full, but the outbreak was evidently dependent upon the unsuspected accumulation of cesspool soil, of the filth from a pigsty, and of the drainage from privies on a piece of waste land screened from observation by the north wall of the establishment. This piece of ground is described by the reporter, Dr. Buckler, as being "one putrid and pestilential mass, capable of generating, under the ardent rays of a midsummer sun, the most poisonous and deadly exhalations." The wind set pretty steadily from the north during the prevalence of the epidemic, and the inmates of the almshouses suffered in exact proportion to the exposure of their apartments to its influence.‡

Again, in Boston, U.S., it appears that wherever the air was impregnated with the miasmata arising from foul privies or other collections of night-soil, there cholera was rife. Dr. Clark describes one place in which 12 deaths occurred in two days, out of a population of fifty; the cause being, in his opinion, the accumulation of all the excrementitious and other refuse matter in its centre. In another locality, equally exposed to the effluvia from fæcal matter in a state of putrefaction, 200 cases occurred within a circle having a radius of a few rods.§

The Cholera Commissioners, in their 'Report on the Outbreak in Newcastle and Gateshead,' refer that painful tragedy to the same class of local circumstances as were found in conjunction with the heaviest mortality from cholera in the metropolis in 1854. They speak of the absence of water-closets and the deficiency of privies, as well as of the existence of dunghills; of privies overcharged from neglect of scavenging, or constructed against the walls of houses, "so as to allow of the liquid filth oozing directly through the walls into living and sleeping-rooms," or "so as habitually to bring these 'poison-pits' close to the windows or doors;" of foul, offensive, obstructed, or ill-constructed sewers; defective inside trapping, untrapped gully-grates outside, as having been evils so common, that even the houses of the upper and wealthier classes of Newcastle and Gateshead were not exempt from their influence.||

* Reports on Asiatic Cholera in Regiments in the Madras Army, from 1828 to 1844, p. 25.
† Loc. cit., p. 4. ‡ Dr. Wynne's Report, pp. 68–75. § Loc. cit., pp. [illegible].
|| See the Report, paragraphs 34, 59, 62, 63, 64, 66, 68, 75, 77, 125, and 127.

The same local sources of atmospheric impurity that thus seem to have been so influential in the case of cholera epidemics elsewhere, are blamed by Dr. Lucas for the outbreak in Brecon, in 1854. The entire town is represented to have been in the most unwholesome condition; but the force of the epidemic was chiefly expended in one district, through which flows the Mandrel, a filthy brook, which, receiving the sewage and drainage of the neighbourhood through which it passes, is little better than a common sewer. The mortality from cholera in the entire town was at the rate of 30·7 in the thousand; but in Bailyglas and Black Boy, the two districts most severely visited, at the rate of 83·0 and 79·7 in the thousand respectively. 'The Mandrel runs more sluggishly past Bailyglas than elsewhere, and here receives the soil direct from such privies as exist in the district. These evils were aggravated by special circumstances about the time of the epidemic visitation.'*

A limited outbreak which took place in the North City Dispensary district at Grangegorman, Dublin, on Sunday, November 5th, 1853, strongly favours the idea that cholera is at least occasionally of local origin, and that the circumstance which we have found so frequently associated with it, is probably its efficient cause. Out of 6 cases, 5 were fatal; four of them in periods of ten, eleven, thirty, and thirty-one hours respectively.

"All the patients lived close to one another; and though the locality where they resided is open, the hygienic conditions of their habitations were extremely bad. In their immediate vicinity, there was a very large collection, in a yard, of street manure and night-soil, collected by carts from the lanes and alleys of the neighbouring parts of the city. No other case of the disease occurred at this time. It did not become epidemic in Dublin for more than ten months after."†

Very striking as is the evidence of the local conditions usually associated with cholera epidemics on shore, that we have thus collected from so many sources, its occasional appearance in an epidemic form among the crews of ships at sea would seem to indicate that the presumed local condition is not required for the evolution of the pestilence; and if not required, that therefore it cannot be a cause of the disease, however much it may aggravate it. Upon this head we possess little positive information; but from the 'Report on the Cholera in the Black Sea fleet,' it appears that some of the vessels were, after the appearance of the cholera on board, less free from such atmospheric contamination than might be supposed. Mr. Rees, surgeon of the *Britannia*, attributes the outbreak on board that vessel, in a great measure, to defective ventilation; and adds, that when a return to port was decided on, the continued violence of the scourge, the crowded state of the middle deck, the discharges from the bowels and stomachs of the sick, and the want of adequate ventilation, had contributed to render the ship a laboratory of pest poison. In the *Albion*, 419 cases of diarrhœa and cholera occurred among a crew of 800, of which 69 proved fatal. The pestilence reached its climax, both on board the *Albion* and *Britannia*, on the 14th and 15th of August, 22 out of 25 persons attacked on board the *Albion*, on the last of these days, having died. The surgeon accounts for this fearful mortality, on the sup-

* See Dr. Lucas's Report, p. 15.
† Third Annual Report of the Commissioners of Irish Poor-Law, p. xxii.

position that the "evacuations from those previously infected had accumulated, and acquired a much more deadly influence from imperfect ventilation, in consequence of the state of the weather." The crews of all the vessels had been more or less on shore, where cholera was at the time prevailing in an epidemic form. Supposing that the earlier cases had received the disease on shore, and that their discharges accumulated between decks in the manner named by Mr. Rees and the surgeon of the *Albion*, there is no longer any difficulty in understanding how, in the hot, close, confined, still atmosphere of a ship's lower deck, atmospheric contamination would arise and become concentrated, so as possibly to induce the subsequent disastrous outbreak.

Cholera having thus prevailed almost exclusively in localities the atmosphere of which has been vitiated by the products of the decomposition of excrement, may we not almost venture to believe this to be the true localizing cause of the pestilence—the "terrene cause" of Dr. Barton,—the other factor to which we referred as a necessary contingent for the causation of pestilential epidemics? The isolated outbreaks, such as that in Tynemouth, already referred to;* that in Dublin, mentioned in the 'Report of the Irish Commissioners of Health;' and others which might readily be adduced; together with the fact related by Dr. Acland,† and other unimpeachable authorities, that cholera may arise without communication with any infected source, are, at the same time, readily explicable by, and afford support to, the supposition. In fact, such occurrences, and the sporadic cases presenting all the essential characters of the epidemic form of cholera which present themselves to our notice in ordinary seasons, are only explicable on the supposition that the poison is capable of production in this country under favourable circumstances. Such an opinion does not necessarily negative the transference of the poison, which, whilst it may thus, on the one hand, arise independently, may, on the other, be the means, under suitable conditions, of exciting a similar form of decomposition when accidentally removed to a fresh locality possessed of the requisite local conditions. Chemical analogy is rather in favour than not of the portability of the leaven, and many of the facts which have been adduced in proof of the presumed communicability of cholera, harmonize well with such a view. Again, the meteorological conditions which have been so frequently found to precede or accompany epidemic visitations of cholera, are perhaps favourable to some particular form of decomposition giving rise to poisonous exhalations; whilst the still, heavy atmosphere, so often prevalent throughout cholera epidemics, will tend to retard the rapid diffusion of such products of decomposition into space, which, thus detained in the immediate vicinage of their origin, will strike with aggravated violence. The strictly local nature of many outbreaks which are often confined to a single house, or a street, or to a small portion of a town, so that most large epidemics are, as it were, composed of a succession of small outbreaks, is also favourable to this view, which, again, harmonizes with Dr. Pettenkofer's observation, that the cholera miasm soon begins to lose its force, and this at very short distances from the place

* British and Foreign Medico-Chirurgical Review, vol. xvii p. 291.
† Dr. Acland's Memoir on the Cholera in Oxford, pp. 40, 78.

of its development, in consequence of its dilution with air. It is indeed true that the chemist has hitherto failed to detect any unusual element in the atmosphere during cholera epidemics, or to discover any peculiar product of decomposition which may be presumed to be the exciting cause of cholera. He has, however, been equally unable to detect the paludal poison, or to discover the nature of the peculiar decomposition of which it is the product.

Strong as is the evidence we have adduced in favour of the influence of season, and of a certain source of local atmospheric impurity in the causation of epidemics of pestilential cholera, we are far from asserting the case as proven. We set it forth merely as that which accords best with the known history of the disease, with the view of directing attention to the probability of cholera being, at least sometimes, of indigenous origin, and to its very constant co-existence in the malignant form with a definite cause of vitiated air. Had we desired it, the case might have been strengthened by additional evidence, and particularly by facts which would negatively tend to show that the removal of the supposed cause has both appeared to check and to avert epidemic visitations. It is certainly remarkable that so many independent observers have referred cholera to fæcal matter in one form or another; and this fact alone seems to show that there is some truth in the opinion. Putting aside the theory, that the recent cholera evacuations are the materies morbi—for no proof of their poisonous character has yet been adduced—our view accords with that of Dr. Pettenkofer and the other German authors, save that we incline to believe cholera may be produced by fæcal decomposition independently of the presence of the evacuations of cholera patients. It accords still more closely with that of Dr. Acland, who, although he in one place says—not apparently as a result of his own observation, but on the authority of Drs. Budd and Alison—"it can scarcely be any longer doubted that the evacuations of cholera patients are capable of communicating cholera,"* elsewhere propounds an hypothesis of the ætiology of cholera which agrees with that set forth in this article; excepting that, whilst we have not attributed the generation of the poison which causes the pestilential form of this disease, to the decomposition of any single form of fæcal matter, the Doctor limits it to that of the evacuations of diarrhœal patients.

"No one doubts," says Dr. Acland,† "that in a cholera period—1st, *persons die of diarrhœa and of choleraic diarrhœa, without passing into cholera;* and, 2ndly, *such cases do oftentimes pass into cholera.* . . . . Now the hypothesis is, that the first group are produced by 'atmospheric influence' (let the general cosmical conditions be so named), without any specific poison; and that the second group are produced by the same atmospheric influence as the first group, operating on discharges from the bowels, and producing a specific poison; the poison capable of acting on the individual who produced the discharges which can be so altered, or on other persons; the discharges innoxious, or incapable of communicating the disease *until so altered;* but when so altered, either within or without the body, capable of distribution through the atmosphere, probably either in a dry or in a gaseous state, and of absorption by the lungs; or capable of solution in water, and of absorption by the digestive organs. Or, more briefly, one cause (the atmosphere) produces the first group of disease, and along with the disease an organic product

* Memoir, p. 73. † Loc. cit., p. 76.

(alvine discharge), which is innocuous until altered by the very cause which produced it, and then it becomes the cause of the second group; so that it might be theoretically, and perhaps truly, said, that if the cause which produced the diarrhœa ceased before the discharges could be acted upon, then they would remain for ever innocuous."

This hypothesis of Dr. Acland's, for he only offers it in that light, tallies with the well-recognised fact that dysentery is frequently produced by inhaling the odour of dysenteric evacuations.* Whilst, however, we are gratified at learning that Dr. Acland has independently arrived at conclusions which approximate in several respects to our own, we are not disposed to limit the generation of the poison, but rather to believe, until more accurate observation shall have determined otherwise, that the "atmospheric cause" acts almost, if not quite, as much on ordinary as on diarrhœal alvine discharges.

We venture to hope that, should unhappily another visitation of cholera occur in this country, the entire subject of fæcal decomposition will be systematically investigated, and that the several views which have been put forth on the subject, and to which we have referred, will, as far as circumstances admit, be tested in a logical and scientific manner.†

Review VI.

1. *Anatomisch Physiologisch Onderzoek over het fijnere zamenstel en de werking van het Ruggemerg.* Door J. L. C. Schroeder Van der Kolk.—*Amsterdam*, 1854. 4to, pp. 90. Met drie Platen.

Anatomical and Physiological Inquiry into the Minute Structure and the Functions of the Spinal Cord. By J. L. C. Schroeder Van der Kolk.—*Amsterdam*, 1854. With three Plates.

2. *Neue Untersuchungen über den feineren Bau des Centralen Nervensystems des Menschen. I. Medulla Spinalis und deren Bulbus Rhachiticus.* Von Joseph v. Lenhossék, Doctor der Medicin, &c., Professor der Anatomie und der gerichtlichen Medicin an dem Kais. Kön. Lyceum in Klausenburg.—*Wien*, 1855. 4to, pp. 70. Mit iv. Tafeln.

New Researches into the Minute Structure of the Central Nervous System in Man. I. The Medulla Spinalis and Bulbus Rhachiticus. By Joseph v. Lenhossék, M.D., &c., Professor of Anatomy and Legal Medicine at the Imperial and Royal Lyceum in Klausenburg.—*Vienna*, 1855. With four Plates.

* Pringle's Observations on Diseases of the Army, fourth edition, pp. 22, 23, 86, 316. Lind on Fevers and Infection, p. 241, London, 1779. Copland's Medical Dictionary, pp. 699, 704.

† The investigation of fæcal fermentation during healthy periods is, however, quite as important as that during times of epidemic visitation. Without an acquaintance with the ordinary products of such decomposition, it is clearly impossible to determine the existence of any specially poisonous product during epidemic periods. Just as the last proof of this article was corrected, we received the Seventeenth Annual Report of the Registrar-General, containing a lucid summary, by Dr. Farr, of the facts deducible from the death statistics of the cholera epidemic of 1854. These facts afford support to the opinion that cholera is, now at least, indigenous to this country, and but an aggravated form of a disease continually present amongst us. "Cholera itself," says Dr. Farr, "has probably always existed in England." Its intensity apparently depends "chiefly on local and meteorological circumstances."

3. *Disquisitiones Microscopicæ de Medullæ Spinalis Textura, imprimis in Piscibus factitatæ.* Conscripsit PHILIPPUS OWSJANNIKOW.—*Dorpati Livonorum*, MDCCCLIV. 8vo, pp. 51. Accedunt tabulæ tres cupro expressæ.

Microscopic Researches into the Structure of the Spinal Cord, chiefly in Fishes. By PHILIP OWSJANNIKOW.—*Dorpat*, 1854. With three Plates.

IN all recent investigations into the structure and functions of the nervous system, the anatomists and physiologists of this country have constantly borne a distinguished, and sometimes a pre-eminent part. Since Sir Charles Bell, in 1811, first surprised the scientific world by the indication of his interesting discoveries, we have had the ingenious and important inquiries of Dr. Marshall Hall, who, if not strictly unchallengeable in all his claims to originality, and if in certain of his views he must yield the palm of priority to Unzer and Prochaska, has at least so largely developed what others had merely foreshadowed, has so enriched the aggregate by additional facts and explanations, and has so enhanced its value by new practical applications, that we must assign to him merit of the very highest order, even if later investigations, dependent chiefly upon more improved modes of examination, should ultimately require modifications of his views as extensive as those which he has enforced upon his predecessors. The names of Reid, Grainger, Swan, Solly, Todd, and Bowman stand also honourably illustrious in this department of inquiry; while, among those who have especially brought the aid of the microscope to bear upon their researches, Mr. Lockhart Clarke has justly attracted considerable attention, as well among our own as among foreign observers. Thus the mysterious and difficult topic has a double interest for us, and we watch its growth with one glance towards the progress of the science, and another to the conspicuous share in its advancement which belongs to our able countrymen.

Looking at the subject as practical physicians, it is pleasing to observe how much of this progress has been due to those who belong to our laborious class. While we admire the energy which finds opportunity in the midst of so many anxious distractions, for investigations so abstruse and so delicate, we cannot doubt that the very habitudes which bring the practitioner into his manifold relations with the manifestations of life, whether in their normal or abnormal aspects, are precisely those which are fitted to communicate a just direction to his inquiries, as well as to supply the appropriate checks, by suggesting a qualification here or interposing a negation there, so as to give circumspection to his steps and weight to his deductions. In this way, while the phases of health supply illustrations which the phenomena of disease limit and define, we look back again to the facts and principles eliminated, either thus only, or with the aid of whatever other means of inquiry, and seek a reflected light to be thrown in its turn upon other morbid phenomena, so as to facilitate that surety of diagnosis of a variety of diseases, without which their proper rules of treatment are rarely obvious, and never irrefragable. It is, therefore, with the aims of the practical physician that we now turn our attention to certain recent researches into the minute anatomy and

the functions of the spinal cord, confining ourselves on the present occasion to the labours of two or three of our continental brethren. If the natural growth of scientific doctrine, with the truly scientific ever slow and hesitating, does not yet entitle us to anticipate any signal portion of that rich guerdon which is still in store for discoverers in this interesting department, the name which heads our list may at least assure us that we shall gather nothing that does not proceed from the careful research of a maturely experienced and penetrating intelligence.

I. Professor Schroeder Van der Kolk does not commence his observations without an acknowledgment of those difficulties which invest his subject, and which have hitherto conduced to so many diversities of result. Having shortly discussed the previous researches and speculations of Ehrenberg, Valentin, Remak, Hannover, Stilling, Volkmann, Wagner, and of Todd and Bowman, he passes to the narrative of his own inquiries. Induced by his success, in the year 1847, in demonstrating a close relation between the peripheral distribution of the sensory and motor nerves, through his discovery of the law that everywhere throughout the body the sensory ramifications of a mixed nerve pass to the surface of the part which is moved by the muscles receiving their motor fibres from the same nerve, so that, while the latter become the instruments of motion, the former supply sensation to the part moved,* he was led to infer, farther, that there must exist an intimate central union between the motor and sensory nerves of any individual trunk, and to seek for the proofs of this union in a more scrutinizing examination of the structure of the spinal cord. Although the result failed to correspond fully with his requirements, still it appeared to him that his investigations threw important light on most of the questions relating to the structure and functions of the cord; and he justly offered them as material contributions to so uncertain a field of research, into which a multitude of vivisections had hitherto introduced more confusion and inconsistency than substantial truth. They were first announced in the Proceedings of the Section for Natural and Medical Science of the Utrecht Provincial Society, towards the close of June, 1848, and reported more fully in the autumn of the same year, before the Royal Institute of the Netherlands.†

We shall not pause to specify the views thus originally promulgated, and now reproduced by the author; or to contrast them with those advanced in the interval by other inquirers. In the latter, Schroeder Van der Kolk discovers many discrepancies, but he regards them as generally confirmatory of his own prior inferences, though framed without these having reached the cognizance of the several investigators. He thus passes in review the researches of Clarke, Engel, Schilling (of Dorpat),

* Tijdschrift der Wijs-en Natuurk. Wetenschap. van de Eerste Klasse van het Kon. Ned. Inst. 1847, p. 44 sqq.

† We have not seen the original Dutch notice (Aanteekchingen van de Sectie-vergadering van het Prov. Utr. Gen., Junij, 1848), or its translation into Swedish by Liedholm (Hygiea, medicinsk och Pharm. Monads-skrift, B. XI., 1849, p. 553 sqq.), referred to by the author; but an abstract, also by Liedholm (Om ryggmärgens function och histologiska byggnad, &c : Hygiea, B. XVII., 1855, p. 593 sqq.), of the later publication, now before us, has been kindly recommended to our attention by Professor A. Retzius, of Stockholm. This we have examined, and with such satisfaction as to its general clearness and accuracy, that we might have adopted it at once, as a lighter labour than forming an abstract of our own, were it not that we prefer, as a duty to our readers, to turn to original sources wherever these are accessible.

and Gratiolet, and adverts to the opposing opinions of Kölliker. Meanwhile, having himself renewed his investigations with improved methods, and having also widened their field, still with the uniform effect of strengthening his deductions, he now proceeds to present to us the whole, as the ripe result of prolonged and reiterated experience; and assuredly as such, and as proceeding from a source so enlightened, they have every claim upon our attentive consideration. It is at this their matured stage that we lay them before our readers.

A primary object was to ascertain the best method of securing such sections of the spinal cord as presented the fittest conditions for a successful scrutiny. After testing the processes of other investigators, and resorting to a diversity of expedients of his own devising, he arrived at the conclusion, that by the following plan he attained the maximum of advantages, with the minimum of contravening defects. The cord, cut into moderate-sized portions, was first hardened in alcohol, and so soon as the requisite degree of consistency was reached, all beyond this injuring the distinctness of the preparations, the fine sections were made by means of a broad, keen-edged razor. Such a section was now laid upon a glass slide, with a little distilled water, and a covering-glass placed over it; the edges of the latter being then alternately pressed down very gently, so as to force the water between the fibres, without breaking up the texture. A milkiness is thus speedily caused, which is to be washed away by a continuous dropping of water at the margin of the covering-glass; and this, with the alternating pressure, is repeated till the turbidity ceases to appear. In this way, by a cautious manipulation are removed the fat globules and the detached molecules which obstruct the transparency. By now holding the glass obliquely, and bringing a current of water against the edge of the covering glass, the latter glides from the object without injuring it. The surrounding water is next wiped away, and a few drops of a concentrated solution of chloride of calcium are applied to the section by means of a glass rod; the covering glass is replaced, pressed down gently, and allowed to remain; and, in the course of half an hour, or even earlier, a degree of transparency begins to be obvious, which gradually increases, so that in eight or ten days all the fibres are defined with distinct outlines and can be readily discriminated from the minute capillary vessels. The edges of the glasses are finally luted together with asphaltum. In this way a number of preparations are made, compared with each other, and the best and most distinct retained. In objects hardened with chromic acid, the author was not so successful in displaying the ganglion-cells and their fibres, as with alcohol. In the use of the microscope, he has derived generally the best results from the employment of a power of from eighty to one hundred diameters.

The author does not enter into any complete description of the spinal cord, but confines his details to the principal facts which have been noted by himself, and which he illustrates by reference to a series of lithographic figures. The reciprocal communication of the multipolar ganglion-cells, by means of their connecting filaments, he has examined in a variety of longitudinal and transverse sections; and he holds that, by means of his preparations, he has demonstrated the nature of this in the most conclusive manner, though not always with equal facility. Sometimes two

ganglion-cells are found adjacent to each other, and connected by a filament of considerable thickness; usually, the connexion is apparent between neighbouring cells, or it takes place between those which are more remote from each other, so that not rarely a connecting thread passes over the nearest, without communicating, that it may unite with another more distant; sometimes the cells are conjoined by more than one individual filament. These ganglion-cells are found most abundant in the anterior horns of the grey substance, as indeed has been remarked by most writers, and chiefly near the points of entrance of the nerves; their most considerable aggregations being in the cervical and lumbar enlargements of the cord, where the grey matter expands into wider dimensions, and the emerging nervous trunks are the most numerous. Besides these ganglion-cells in the anterior horns, others occur in the posterior horns also, but of smaller size, and fewer in number. In so far, most of the author's observations here agree with those of Clarke, Schilling, Gratiolet, and Kölliker: but he has met with another unvarying group of ganglion-cells, which appear to him of essential importance, and which have not been adverted to by others; unless, as he considers probable, they have been noted by Kölliker, without, however, his assigning to them their just relations. These cells lie together in a small and compact group, among the radiations of the posterior grey commissure, into which their filaments evidently pass. They differ from those in the anterior horns, through the smaller number of their threads; many of them are oblong, triangular, and, where least in size, they are commonly very closely arranged. There are, farther, isolated ganglion-cells between the white substance, or longitudinal fibres, of the cord. These cells are in small number, lie in the lateral expansions of the grey substance, and chiefly in the vicinity of its mass, and have been remarked by Clarke.

From all this, the author coincides with Clarke in deducing that several distinct vertical columns of multipolar ganglion-cells exist in the cord, extending throughout its whole length: that of these cells the most considerable are in the anterior horns; that next are those at the side of the posterior commissure; then those in the grey substance between the anterior and posterior horns; and lastly, those in the posterior horns themselves, which rank as the smallest. But these columns of cells must not be regarded as subsisting independently of each other: on the contrary, they are all of them more or less closely connected. Neither are they, viewed in their longitudinal direction, of uniform expansion. Not only are they larger and richer in cells at the cervical and lumbar enlargements, but the proportion of cells increases at all points where the roots of nerves penetrate into the cord and its grey substance; so that they constitute thus a series of more or less dependent clusters, placed longitudinally above each other. Their connexion with the roots of the nerves, like their reciprocal communication, is demonstrated by means of both transverse and longitudinal sections; the latter as near as possible to the entrance of the anterior roots, in the direction of the anterior horn of the grey substance, or rather parallel with the course of their nervous fibres. By a transverse section he has succeeded repeatedly in tracing the nervous threads distinctly and uninterruptedly from without into the horn; dividing themselves into thicker or thinner fasciculi,

some of which pass along the outer margin of the horn, while others distribute themselves through its substance. At the entrance into the grey substance lie usually a few multipolar ganglion-cells, from which it is sometimes possible to follow excentric filaments passing into the nerve-roots, or lateral radiations, as has been very clearly represented by Clarke, although the importance of the fact escaped his cognizance. But the connexion of the nerves with the ganglion-cells, or rather their origin from them, on the anterior or motor side, he has best demonstrated by means of longitudinal sections. Upon the whole, he judges that there can remain no doubt that the roots of the motor nerves spring from the cord, and specially out of the ganglion-cells of the anterior horn, which are mutually conjoined into a plexus, and frequently separate themselves into more or less distinctly segregated groups.

The leading question remained—in what way are these motor nerve-roots connected with the brain, through the medium of the mesh of ganglions into which they pass? That the anterior medullary fibres are the channels for the operation of the will on the motor nerves, the author receives as beyond dispute. But the connexion between the medullary fibres and the grey substance is not so obvious. To demonstrate this, it is necessary to examine the disposition of those transverse fibres, which are seen among the longitudinal fibres on all sides, as radiations, more or less divaricated, from the grey matter. From the divergent and contorted course of these fibres, it is rarely possible to trace them: but the author considers that he has shown that they form a curve, and pass in the direction of the longitudinal fibres; having especially substantiated this by means of a very fortunate longitudinal section from the antero-lateral column, in which the innermost longitudinal fibres were seen moreover to bend towards the grey substance, and pass into the ganglion-cells. As the general result of these and of other observations, he thinks it manifest, that while, as all writers have remarked, the longitudinal white cords maintain for the most part an uninterrupted parallel course, still that transverse fasciculi, issuing from the grey matter, separate and spread themselves among the white substance, and unite with a portion of its fibres; so that the longitudinal fibres, as channels of the will, communicating with these transverse fibres, convey the influence of the will to the ganglion plexus out of which the motor nerves take their origin.

The investigation of the structure of the posterior horn, and of its intrant nerve-roots, presents still greater difficulties. After again briefly narrating the views of other observers, Professor Schroeder Van der Kolk proceeds with the result of his own researches. A thin longitudinal section, at the entrance of the posterior roots, shows that a portion of these penetrates the cord, but immediately afterwards curves upwards in the posterior longitudinal column. The fibres run parallel with the white fibres, with which they afterwards unite; and are then covered by those of the sensory nerves having a higher origin, so that they may be said to lie imbricated under each other. Besides these fibres, passing thus longitudinally in the posterior column, other transverse fibres, issuing from separate fasciculi, dip towards the centre, or the posterior horns. These are best seen in a transverse section, taken at the level of a nerve-root passing into the cord, and are especially fine and delicate where traversing

the gelatinous matter of the posterior horn. The author has traced them to groups of ganglion-cells, without, however, being able to show any actual communication. In addition to these nerve-roots, there occurs here another description of fibres, which appear to the author not to have attracted sufficiently the attention of other writers. They pass round the whole posterior horn like a band or girdle, and appear to proceed chiefly from the transverse radiations which here, as in the anterior horns, intersect the longitudinal column, and spread out with many branches at those parts of the cord where no nerve is seen to enter. These encircling fibres possess numerous small, generally oblong, ganglion-cells, and communicate with fibres issuing from the posterior grey commissure. It thus appears, that at the posterior part of the cord there are two descriptions of nerve-roots, of which the one ascends immediately in the white substance, and appears to proceed directly into the brain, constituting undoubtedly the channel of sensation; while the other roots traverse the white substance towards the posterior horn, through which they penetrate, and, mingling in part with the encircling fibres, lose themselves apparently in the ganglion-cells of the centre of the grey matter between the anterior and posterior horns. The latter form thus, in all likelihood, the apparatus of reflex action, and direct the stimulus through the group of ganglion-cells, with which they appear to be connected, into the anterior cell-groups from which the motor nerves derive their origin.

If a longitudinal section be now made through the posterior horn, it is seen obviously that the so-called gelatinous substance is composed of fine, translucent, longitudinal fibres, running parallel with each other, and much more slender than the white, ascending medullary, or sensitive fibres. These delicate fibres exist in the greatest abundance in the cervical and lumbar enlargements of the cord, and do not appear to pass uninterruptedly upwards as sensory fibres, otherwise the posterior horn could not present smaller dimensions in its dorsal than in its lumbar portion. But if we advert to the origin of the motor nerves from groups of ganglion-cells, and consider that these groups must be reciprocally connected in order to bring the different muscles into co-ordination of action; and that farther, during certain conditions of irritation of the spine, a stimulus may excite many, or even the whole, of the nerves of the cord simultaneously into convulsive energy, and thus extend the reflex motions to parts remote from each other,—it becomes more than probable that these longitudinal translucent fibres are connecting fibres, that is, that they serve to unite together the different cell-groups throughout the cord, and are thus the peculiar agents for the co-ordination of the movements.

In his views of the texture of the commissures of the cord, the Utrecht Professor chiefly coincides with those of Schilling. The anterior commissure is essentially distinguished from the posterior by the decussation of its fibres. After their intersection, these fibres are deflected, and run in part along the margin of the anterior fissure, interlacing themselves within the white substance; and in part enter the inner edge of the anterior grey horn, where they mingle with the encircling fibres already described, which spread themselves thence in the medullary columns, and join the longitudinal fibres. They are not seen to pass directly over into the roots of the anterior nerves. Their function is probably to maintain

a connexion between the motions of the right and left sides of the body, through the intervention of the middle group of ganglion-cells, acting upon that in the anterior horn. The posterior commissure varies greatly in dimensions, being very broad in the lower part of the lumbar portion of the cord, much smaller in the dorsal, and again broader in the upper cervical portion. Its fibres have a parallel course, without intersection; those nearest the central canal passing into a group of ganglion-cells, and the others traversing from the middle of one side to the middle of the other side, where they appear to terminate in the central cluster. In this, as in a more general focus, are united the reflex fibres, the encircling fibres of the posterior horn, and the posterior commissure. Between the two commissures is the central canal, which the author maintains to be an open channel, a continuation of the fourth ventricle, and lined with epithelial cells.

Such is a condensed recapitulation of the researches of Schroeder Van der Kolk, to which it has been our endeavour to give all the distinctness attainable without the advantages of the accompanying illustrations, or of those minor explanatory details by which he himself elucidates the steps of his investigation. From a general review, he considers himself entitled to deduce the following as the sum of his conclusions :—

1. The ganglion-cells, especially in the anterior horn, are joined reciprocally by more or less divaricated connecting filaments, and thus unite into more or less distinct groups.

2. From the ganglion-cells, especially in the middle and anterior portions of the anterior horn, arise the motor fibres, which unite at the margin of the grey substance into one, or several, contiguous nervous bundles, quitting the cord in a transverse direction, to compose the roots of the motor nerves.

3. Along the outer margin of the anterior horn, run encircling or marginal fibres, which have their origin from the radiations expanded in the longitudinal columns, and are connected with the ganglion cells situated in considerable numbers on the outer edge of the horn. Those cells communicate with others placed more profoundly; and thus ultimately with the group of ganglion-cells from which the motor nerve derives its origin.

4. The anterior longitudinal columns are composed of white medullary fibres, for the most part parallel, which pass into the already mentioned transverse radiations, and through them convey the influence of the will to the ganglion-cells in the grey substance. The longitudinal fibres, placed the nearest adjacent to the grey horn, bend immediately round, in order to attach themselves to ganglion-cells.

5. The posterior nerve-roots include two descriptions of nervous fibres: those for sensation proper, and those for reflex. Hence the greater thickness of the posterior roots compared with the anterior.

6. The sensory nerve-roots, immediately after their entrance into the cord, proceed upwards along the posterior columns, to reach the brain as the seat of perception. They do not penetrate into the grey matter.

7. The fibres for reflex action pass transversely towards the posterior horn, and make a number of interlacements between the longitudinal fibres, while a portion of them proceeds through the so-called gelatinous

matter of the posterior horn into the middle of the grey substance, where they appear to connect themselves with the ganglion-cells. Possibly they send communications also to the encircling fibres, which everywhere surround the grey posterior horn like a band.

8. These encircling fibres proceed in great part from the radiations, which spread themselves from the posterior horn into the medullary matter; they surround the horn, and at its basis bend themselves from either side towards the middle, to terminate in the group of ganglion-cells which receives also the reflex fibres. Among these encircling fibres are scattered a number of generally oblong ganglion-cells; while a few ganglion-cells are also found in the gelatinous substance, especially near its centre.

9. The posterior horns of the grey substance consist principally of very slender longitudinal fibres. Now, as these horns are at least five or six times thicker in the cervical and lumbar enlargements than in the dorsal portion of the cord, it follows that these delicate fibres must exist in far greater proportion in the one situation than in the other; and therefore do not pass unbroken throughout the whole length of the cord, but terminate for the most part in the cervical and lumbar enlargements, where reflex actions and motions are most abundantly excited and combined. They appear thus, by their longitudinal direction, to connect more or less closely several cell-groups placed above each other, and constitute therefore longitudinal communication-fibres (*communicatie-draden*).

10. The posterior commissure, composed of grey fibres, passes partly into contiguous ganglion-cells, partly into cells placed in the middle of the grey substance, and partly becomes connected with the encircling fibres of the posterior horn.

11. The anterior commissure forms a decussation: its fibres take a direction forwards, in part to terminate directly as radiations betwixt the innermost, anterior, longitudinal cords; in part to pass over to the inner edge of the anterior horn, where they unite with the encircling fibres, which derive their origin from the radiations, as already described.

12. The fibres, as well of the anterior as of the posterior commissures, have no connexion with the roots of the nerves directly, but probably are associated with the anterior through the medium of connecting filaments between the different groups of cells; and both commissures consist of grey fibres.

13. A canal exists persistently within the cord, having its interior lined with epithelial cells, and appearing occasionally to contain an albuminous fluid. It is of smaller calibre in man than in most animals.

Having thus detailed his views with regard to the minute structure of the cord, Schroeder Van der Kolk proceeds to discuss the physiological inferences to which they appear to lead. He considers that he has thoroughly established, that the moter nerves have their origin within the cord, and especially from the multipolar cells in the anterior grey horn; and argues against the notion of their arising directly from the brain, as surrounded by many impossibilities. The influence of the will is merely conveyed to them along the anterior and lateral columns; and it thus follows, that the number of longitudinal medullary fibres, which serve as

conductors from the brain, may be relatively small, and are only required to tally with the varying number of groups of ganglion-cells in the anterior horn. These, again, are necessarily in relation with the individual muscle, or part of a muscle, or group of muscles, which enters into any simple or combined movement. Hence, if we compare, by means of transverse sections, the thickness of the antero-lateral columns at different heights of the cord, we find that these only enlarge slightly in their course upwards. In the posterior columns, on the other hand, it has been seen that the proper sensory fibres bend immediately upwards on their entrance into the cord, and that thus these columns must contain as many fibres of this description as the sensory nerves supply. Accordingly, an ascending series of transverse sections shows that the posterior and postero-lateral parts of the cord become, by the successive increments of the sensory nerves, much thicker in their passage upwards than the anterior columns. This point is well illustrated by the very exact representations in the plates of Arnold. The necessarily greater abundance of groups of ganglion-cells, in situations where there are the greatest requirements for simple and combined movements, explains also the varying degrees of thickness of the anterior horn of the cord, and especially the existence of the maximum at the cervical and lumbar enlargements.

But, besides the proper sensory fibres, which proceed upwards, it has been seen that the roots of the sensory nerves supply other transverse fibres, which, splitting into different fasciculi, pass to the posterior horn of the grey substance, and appear to lose themselves in the group of ganglion-cells situated in the midst of the grey substance betwixt its horns. These Schroeder Van der Kolk recognises as reflex nerves. He does not, however, concur with Marshall Hall in admitting a special system of excito-motor nerves: that is, of particular nerves which produce also the motion of which the reflex sensation is the stimulus. This, he considers, is a hypothesis which stands upon no assured basis. It is enough that the roots of the motor nerves receive the excitement to action from the group of ganglion-cells, whether that be originally communicated through the will anteriorly, or by reflex posteriorly. We may thus imagine the group of ganglion-cells as a battery with two poles, or rather as a battery capable of being charged in two directions: the one pole is in connexion with the channels for the influence of our will; the other, through the medium of different combinations of ganglion-cells, is in relation with the reflex fibres; so that an individual group becomes susceptible as well of the stimuli of psychical as of physical agencies. As we must admit, under this idea, two descriptions of nerves in the posterior roots, those for sensation and those for reflex, we find an explanation of their comparatively greater thickness, which is known to be fully double that of the anterior roots. According to this view, all reflex action takes place by a definite course, which guides its operation. This is further regulated through the instrumentality of the longitudinal slender fibres of the posterior horn, which the author conceives to be communication-fibres, bringing the different groups of ganglion-cells, as well as the reflex nerves, into reciprocal connexion, so as to account for the occasional diffusion of the action over remote organs, or over combinations of movements, especially in states of great irritation of the cord, as in the attacks of the

epileptic, or under poisoning from strychnine. In animals destroyed by strychnine he has found considerable congestion, with small extravasations of blood, in the grey substance; a pathological fact which he regards as confirmatory of his views.

It has been shown that the groups of motor cells, as they have been designated for the sake of distinction, out of which spring the motor nerves, possess, as it were, two poles; that is, that they are connected on the one side with the conductors of our will (anterior columns), and posteriorly with the reflex nerves, through the medium of other ganglion-cells. If, now, these reflex nerves be connected with a number of groups of motor cells by means of the communicating fibres, so that, by the instrumentality of reflex, a co-ordinate movement, as a leap, may be effected, we are entitled also to consider that, through the medium of the anterior fibres, conducting the will, those groups out of which a combined or determinate movement arises may with equal facility be brought into action. The cause of the co-ördination of the muscular action is thus, as Volkmann rightly judged, in the spinal cord, and not in the cerebellum; otherwise it would have been impossible to witness a determinate co-ordination of reflex movements in the frog, after decapitation. The commissures, connecting together the right and left divisions of the cord, he considers also to be more or less closely related with the function of reflex. The fibres of the posterior commissure appear to be implanted, in part, in the same ganglion groups in which the reflex fibres terminate, and in part in the small groups of ganglion-cells at the side of the central canal. It is probable, therefore, that these fibres serve for the lateral reflexion, by transmitting the impression received by a group of ganglion-cells to those on the opposite division; while the fibres of the anterior commissure serve rather to maintain the harmony and balance of our voluntary movements on either side of the body.

Much has been disputed with regard to the question of the sensibility or insensibility of the grey matter. According to the supposition of the author, the grey matter of the cord avails solely for motion, the posterior portion being subservient to the reflex function and to the co-ordination of motion: while sensation is transmitted upwards exclusively by the posterior and lateral medullary columns, and has probably its proper centre in the medulla oblongata. In the medulla oblongata also is probably localized the centre from which the more universal reflex movements and convulsions take their origin; and it is to its condition, therefore, that experience has convinced him the physician should chiefly direct his attention in cases of epilepsy, a recourse by which he has frequently succeeded, where the disease has not been of too long duration, in procuring a recovery, through the means of derivative applications to the nape of the neck. The pathological change which results from protracted epilepsy, he has reasons for believing to be an induration of the medulla; but this subject he hopes to have further opportunities of pursuing.

As a summary of the chief points in his physiological deductions, Schroeder Van der Kolk finally offers the following propositions:

1. The different primitive filaments, which distribute themselves as motor nerves in a muscle, or muscular apparatus, appear to arise from a group of reciprocally associated ganglion-cells; they receive the influence

of the will along the anterior white columns, and the transverse or radiating fibres connected with these, which pass into a similar group; and this influence, by diffusing its stimulus equally over all the cells of the group, produces a simultaneous and equivalent action in all the motor threads emerging to constitute the nerve.

2. The number of these anterior fibres, the conductors of our will, must thus be in correspondence with the number of cell-groups, and the different combinations of which these are capable, and consequently is much smaller than the number of medullary fibres for the sensory nerves, in the posterior column; so that, by the continual accession of new sensory nerves, the white medullary matter at the back part of the cord increases more in thickness in its course upwards than the anterior portion, a fact fully demonstrated by the appearance of the cord at different transverse sections.

3. Where a larger number of nerves, to be distributed to muscles, issues from the cord, as for the extremities, there must necessarily exist also a larger number of the cell-groups from which they arise; and hence it is that the anterior grey horns in the cervical and lumbar enlargements are so much thicker than in the upper part of the neck or in the back.

4. In animals exercising only the simpler muscular movements, and in fishes, we have a more slender cord; and the grey substance, as well as the ganglion-cells, is less abundant where the requirements for combinations of movement are also less.

5. The reflex movements do not take place by transilience or transverse conduction, but the reflex nerves appear to terminate, partly in a central group of ganglion-cells more or less closely connected with the various groups of motor cells, and partly in the fine longitudinal fibres of the posterior horns.

6. The posterior horns of the grey substance, through which probably the different groups of ganglion-cells are mutually connected, appear to serve chiefly for the co-ordination of the movements produced by reflex. These movements are more or less general, in proportion to the more or less irritated condition of the grey substance, or of the ganglion-cells.

7. Through their connecting filaments, the groups of motor cells appear to be so conjoined, that, just as merely a stimulus to a single toe suffices in a frog to produce, through reflex, a co-ordinate movement or a leap, so merely an impression is possibly requisite to produce also a strictly determinate movement, such as a step, which can then be modified, according to circumstances, by means of separate impressions on each of the cell-groups. The source of the co-ordination of the movements is seated in the cord, and not in the cerebellum.

8. The transverse commissures seem subservient to the preservation of the harmony of the movements between the two sides: the anterior, apparently more in connexion with the channels of the will, maintains the harmony of the voluntary motions; while the posterior preserves that which is involuntary and by reflex, the equilibrium of the body, &c.

9. Both the horns of the grey substance appear to stand in the most absolute relation with the function of motion: the anterior as its direct

source; the posterior as rather for reflex and co-ordination. Neither shows evidence of being endowed with sensibility.

10. The medulla oblongata is apparently the general middle point where the reflex influence is transferred to either side, and upon the irritated condition of which depend all widely-diffused spasms, as convulsions, epilepsy, &c.

In an appendix, Schroeder Van der Kolk enters into some details regarding certain more recent views of Rudolph Wagner, and others, which, though independently made, he considers confirmatory and illustrative of his own observations. These we forbear noticing more particularly for the present; contenting ourselves with concurring in his recommendation, that the highly-interesting observations of Schiff and Clarke, on the anterior and lateral portions of the cord, should receive the attentive consideration of other inquirers.

II. The division of inquiry which has been occupied, and not unworthily, by Lenhossék, differs materially from that which has been so ably cultivated by Schroeder Van der Kolk. Laboriously comprehensive in his details, these, however, are, with the former, wholly of an anatomical description; and we have therefore neither scope for that unity of design, nor for that originality of conception, which sustain our interest in the discussions of the other, and leave us, in the apparently logical development of his hypothesis, a sense of the gain of a solid acquisition to science, or at least of a vantage ground from which the physiologist may proceed more confidently towards ulterior researches. Besides, let us confess, the style of the author is too much that labyrinth of inverted periods and intercalated clauses which seems to linger in Austria, even with some of its most eminent writers, after it has begun to disappear from the other great fields of Teutonic literature; and which, from its involvement and perplexity, is intricate enough in itself to be peculiarly unfitted for the exposition of intricate textures. It is already difficult, where the minute structure of the central nervous system is considered as a series of detached parts, to trace the order of these, and unite them into the clear conception of a connected whole: but the difficulty is greatly increased where the complexities of language are added to the complexity of subject, and a sense of confusion arises, from which the mind is apt to turn aside, and pause, with a feeling of fatigue and dissatisfaction. A little effort, however, will conduct us through these perplexities, and will enable us to extract briefly such portion of our author's observations as relate more immediately to the structure of the cord, and therefore connect themselves more strictly with our present topic. Meanwhile, we have to thank him for the intelligent labour of his researches, and leave their success to be judged by the abstract which we subjoin.

Dr. Lenhossék's paper was read before the Imperial Academy of Sciences of Vienna, in 1854. His investigations were made by means of sections prepared according to the second method of Clarke, in employing which he congratulates himself upon his remarkable success. Finding advantages in chiefly using the lower magnifying powers of the microscope, he points out the danger of error in passing at once the chasm between an observation by the naked eye and another by means

of those higher powers of the microscope so generally selected, and suggests the propriety, at least, of adopting progressive advances. The grey substance of the cord he describes as constituted by an amorphous, transparent basis, in which are imbedded the ganglion-cells. These are of two descriptions: of which one is universally diffused, while the other, differing essentially from this, is found in certain situations only, and admits of being again divided into two kinds, according as its groups occur in clusters, or scattered. The former, along with the hyaline basis, composes the proper substance of the grey matter. The latter includes a description of cells which are for the most part of a fusiform shape, which present regularly their long diameter in a line parallel with the axis of the grey matter, and which are often placed behind and above each other, so that they are brought into contact both by their surfaces and their extremities. They lie principally in the anterior horns: but they are placed more laterally, and between the anterior and posterior horns, in the lumbar enlargement; above which, and for a short distance only, they predominate in the posterior horns. Hence the representation, by Clarke, of an individual vesicular column, he considers unfounded. The most voluminous of the clustered groups occur in the cervical and lumbar portions, and produce the enlargements in these situations. These cells are conspicuous by their deep-yellow pigment, their great size, and the distinctness of their processes, and constitute what have been regarded by Müller as the proper ganglion-cells of the cord. Both descriptions of ganglion-cells, in as far as has been ascertained by the aid of the best instruments, are multipolar; and both present the most manifold intercommunications, whether amongst themselves separately, or between the two varieties reciprocally, so as to be connected in a continuous chain from the extreme point of the *conus medullaris* to the brain. The scattered or isolated groups of ganglion-cells occur first in the medulla oblongata.

Longitudinal fibres, extending upwards into the brain, cannot be distinctly demonstrated within the grey substance; and, if this have been sometimes imagined, it has been chiefly through a misconception, owing to the surprising distance to which a filament often proceeds, passing in its course other intermediate ganglion-cells, before uniting itself to a process of that with which it becomes ultimately connected. The primitive nerve-fibres, on the other hand, speedily quit the ganglion-substance, proceeding uniformly downwards at an angle of more than thirty degrees to the spinal axis, and unite to form the roots of the nerves beyond the periphery of the cord. In his description of the course of the fibres of the white substance, he sustains generally the views of Clarke; and he takes occasion to point out, that, as the white substance on either side of the cord is completely separated by the fissures, there can be no opportunity for the decussations, recently generally admitted, of either the anterior or posterior medullary columns. The histological elements of the white substance, up to the medulla oblongata, are longitudinal fibres; which form slight undulations where they are pressed aside by the central tracts of the primitive nerve-fibres, in the passage of these outwards to form the roots of the nerves. He maintains the view, that the primitive fibres of the nerve-roots, whether motor or sensory, simply traverse the white substance; and that no portion of them bends itself upwards to proceed to

the brain along with the longitudinal fibres. The primitive fibres of the medullary substance are more slender than those of the roots of the nerves. They appear to issue at very acute angles from the grey matter, and probably proceed from the free cell-nuclei of Kölliker. The central canal of the cord he considers as permanently open, and describes it as lined with cylinder-epithelium.

The primitive fibres of the roots of the nerves proceed, as we have already noted, from the grey, or ganglion-substance, and their course is direct through the medullary matter, without giving or receiving communication. Their immediate origin from individual processes of the ganglion-cells is often, but not in the majority of instances, very distinct, and they arise from both descriptions of cells; the purely motor fibres springing from the anterior horns, and the purely sensitive from the posterior horns, of the grey substance. By the central relations of the general nerve-roots are constituted four distinct anatomical systems: 1st, the anterior, or purely motor system; 2nd, the posterior, or purely sensory system; 3rd, the radiated system, or central portion of the roots of the plexus of the pia mater; and 4th, the lateral, or mixed system, which includes the two upper roots of the accessory nerve of Willis, the par vagum, &c., and has its chief connexions with the medulla oblongata and mesocephalon. The fibres of the radiated system distinguish themselves essentially from all the others, by their passing from the extremities of the *processus reticulares*, or from thread-like processes emanating from the grey substance; by their diverging outwards on all sides; and by their forming an angle with the spinal axis similar to that of the sensory and motor tracts, but opening upwards, or in a reversed direction. The anterior spinal nerve-roots derive their origin from the proper ganglion substance, but especially from the great ganglion-groups in the anterior horns. A portion of the elementary fibres passes uniformly, on either side, from the more internally situated of these groups, to the anterior horn of the opposite side, traversing thus the commissure placed in front of the central canal, and crossing each other at very acute angles. The posterior, or sensory nerve-roots, derive their primitive fibres from the posterior horns of the grey matter; chiefly from the proper ganglion substance, and only partially from the great ganglion groups. They have no direct communication with the anterior spinal roots; but are brought into close relation with them by means of the middle layer of the ganglion groups, as these are seen to dispense filaments in both directions. The fibres also decussate with those of the opposite side, by means of the commissure passing behind the central canal; but this at an angle so acute that their course appears to be parallel, and the intersection frequently escapes observation. These, like all the other elementary fibres, stand as little as those of the anterior spinal roots in connexion with certain separate divisions of the medullary substance of the cord.

The fibres of the radiated system, emerging from the surface of the cord at innumerable points, throughout its whole length, and passing into the pia mater, form the nervous plexus of the pia mater described by Purkinje. Their distribution afterwards is uncertain, but is probably chiefly into the arachnoid membrane of the cord. The appearance of the ganglion-cells in this plexus, partly interposed between the primitive

fibres of the fascicules, and partly attached to their exterior, is a peculiarity to which the author directs attention, as suggesting a probable relation between the function of these nerves and that of the sympathetic. On the other hand, their origin from both the anterior and posterior horns of the grey substance, as the alleged centres of both the motor and sensory functions, appears to denote their twofold capacity of action, and to explain the convulsions, and indications of suffering, remarked in living animals on irritation of the pia mater. The author, however, takes occasion to point out here the difficulty of isolating the necessary conditions, in all experiments made by means of vivisections, with a view to determining the special functions of the different portions of the cord; and accounts thus obviously enough for the heterogeneous and contradictory results arrived at by different inquirers. In his views of the origin of the spinal accessory nerve, he agrees in the main with Clarke. With the exception of the two upper roots, which appear to stand more immediately in relation with the par vagum, he regards the primary origin and central course of its root-fibres as completely identical with those of the radiated system. In this nerve also we have interposed and attached ganglion-cells similar to those of the plexus of Purkinje, but of larger dimensions. As the grey substance does not present any arrangement of a proper system of fibres, its horns cannot justly be designated as cords: neither can we, unless very partially with reference to the portions involved within the *processus reticulares*, admit a division of the white substance into subordinate cords, instead of retaining merely the more general division into columns; its fibres pursuing a longitudinal and parallel course uninterruptedly to the brain, as it were in a mass, and without being further separated by any distinct grooves or fissures.

III. The Memoir of Owsjannikow appears in the form of an Inaugural Dissertation, presented to the Faculty of the University of Dorpat. While he gracefully acknowledges the aid and encouragement in his researches conferred upon him by Professor Bidder, there remains enough, in the details of his investigations, and in the clearness of the exposition of their results, to manifest the great extent of his individual merit and success; and we trust that this able treatment of his topic may secure for him a reception well fitted to undeceive him in the modest expression of his belief, that he has selected a theme likely to attract few readers.

Preparing his objects by hardening them in a solution of one part of pure chromic acid in two hundred parts of water, and afterwards slicing them longitudinally or transversely into the necessary thin sections, he points out the advantages he has derived from selecting the class of fishes as the subjects of his investigations, owing to the singular transparency and distinctness under the microscope of the segments thus procured. The fishes whose spinal cord he selected for examination, were the Petromyzon fluviatilis, the Ammocœtes branchialis, the Lucioperca sandra, Esox lucius, Salmo salar and trutta, Accipenser sturio and ruthenus, Thymallais velifer, Abramis brama, Leuciscus jeses, Silurus glanis, Gadus lota, &c. In the two first of these, where the spinal cord is flat and riband-like, a longitudinal section presents only longitudinal fibres, nearly parallel to each other, running in the direction of the axis of the cord, occupying the whole of its periphery, and corresponding to

the white substance in the higher animals. Within these longitudinal fibres there is a broader column of substance, deeper in tint, which is found to be replete with cells. These are in shape somewhat fusiform, or approaching to triangular and are placed with their long axis in the direction from within outwards. Still nearer the central canal, are observed other longitudinal fibres, of considerable comparative breadth, which have been described by Müller; and among these are found large and round ganglion-cells, each sending out two filaments, which appear to divide multifariously among the fibres in which they are immediately imbedded. The nuclei of these circular cells are of greater magnitude than those of the fusiform cells, but the nucleoli are similar in both. From each of the fusiform cells, a filament is seen to proceed, which traverses the outer longitudinal fibres, and emerges to form part of the posterior root of the spinal nerves. A filament, which proceeds to join the anterior root, is better observed in a transverse section. A third branch, also best seen in transverse sections, passes to the other side, to be attached to a cell there, and to form a commissure; and a fourth ascends upwards towards the brain. Sometimes also a fifth branch is observed to issue from a cell placed transversely, the precise course of which the author has been unable to determine, although once he succeeded in tracing an apparent junction with one of the round cells; and he indicates the reality of this conjunction as an important topic for further inquiry.

Proceeding in his examination of the other fishes, whose spinal cord is more funicular in form, he points out the position of the ganglion-cells as constantly in the grey substance; and as it were imbedded in a stratum of cellular tissue, from which that substance derives its colour. In transverse sections, he meets with a corroboration of his previous observations: filaments are seen to emerge from each cell in three directions; one towards the anterior nerve-roots, another to the posterior nerve-roots, and a third inclining inwards, to pass before the central canal, and unite with a cell on the other side of the cord. In longitudinal sections, and in tracing the root of the nerve inwards, its elementary fibres are seen to bend upwards among the ascending longitudinal fibres, and then to connect themselves with the cells. From each of these another filament now passes, taking first a reversed direction upwards, or in a line gradually receding farther and farther from the central canal; and then, coming finally into mutual contact, they form together the white substance of the cord, the fibres of which pursue a parallel course till they reach the nerve-cells of the brain. Hence it becomes easy to explain the progressively greater abundance of these longitudinal fibres, and the consequently greater thickness of the white substance, in the upward course of the cord. Grouping together the results obtained from both sections, he forms the conception of a cell, or association of cells, seated in the grey substance, giving origin to four filaments, pursuing each the course which has been described: but whether the cells communicate reciprocally with each other in the separate divisions of the cord, by means of a fifth filament, he leaves to be considered only as a physiological probability, if not as a necessity; and points to the shape of the cells, which sometimes gave indications of more than four

emergent branches, as lending strength to his surmise, that such an intercommunication actually takes place, although he has been unable to substantiate it by visual proof.

From the sum of the investigations which we have thus briefly, and but in part, indicated, he deduces the following conclusions:

1. All the fibres of the spinal nerves which enter the cord become connected with ganglion-cells.

2. To each ganglion-cell extends one filament from the anterior spinal nerve-root, and another from the posterior; while a third serves as a commissure between the separate sides of the cord.

3. From each cell a filament ascends to the brain, the aggregate of these constituting the white substance.

4. The fundamental mass of the cord, containing the cells and filaments, is composed of areolar tissue, which, placed in the greatest abundance around the central canal, and freely pervaded by bloodvessels, imparts its peculiar colour to the grey substance.

5. The gelatinous substance, or substance of Rolando, is constituted by areolar tissue.

6. The alleged cells interspersed in the posterior horns and in the gelatinous substance are merely the corpuscles of the areolar tissue.

7. The axis-cylinders are of a round figure, and consist of the same substance as the ganglion-cells.

8. The axis-cylinders in the grey substance are provided with a peculiar membrane, which encircles also the ganglion-cells, and is distinct from the fundamental mass of areolar tissue.

9. In the spinal cord of the Petromyzon and the Ammocœtes the axis-cylinders are naked, and receive no proper covering from the areolar tissue in which they are placed.

10. In these fishes, round ganglion-cells are found close to the broad fibres, and send out branches which split into multifarious divisions.

11. The spinal nerves possess anterior and posterior roots in both these kinds of fishes.

The author has added a few notices of the results of his observations on the spinal cord of man. The central canal he describes as patent and empty, and beautifully lined with epithelial cells. He denies the existence of ganglion-cells in the posterior horns, and especially towards their apices; as well as that of cells around the central canal, alleged to exist by Kölliker, who, he thinks, has been deceived by the presence of the epithelial cells and the areolar corpuscles. Like Schilling and others, he has observed the fibres from the ganglion-cells passing into the longitudinal fibres of the white substance; and he maintains that none of the cells are apolar. The principal substance, or the axis-cylinder of the nerves, is identical with that of the ganglion-cells. The quadripolar cells, or quinquepolar, as he is prone to regard them, he considers to be subservient to the reflex movements; while the multipolar transmit the influence of the will, and are chiefly seated in the brain. We need not dwell upon one or two pathological inferences advanced by the author, because they seem to us to depend upon that vague kind of hypothetical reasoning, of which any quantity may be employed in medicine, but unfortunately in any direction, to prove or disprove at pleasure.

We should have brought into collation here the results of the researches of M. Brown-Séquard, had they not been already so recently noticed in the pages of this Review. It will suffice for our present object to observe, that we do not yet see sufficient grounds to admit them as so wholly subversive of all previous doctrines of neurology, as the French commentators allege regarding them. Indeed, upon the whole, on reviewing the doctrines laid down in the several able treatises of which we have just concluded an abstract, we are glad to discover tokens, not, certainly, of an overthrow of all our previous conceptions on the subject, but rather of a steady advance, and a reciprocal confirmation of those principles which have been gradually developed as the results of the more recent investigations of our micrographers and physiologists. Though there are undoubtedly, as might have been anticipated in so difficult a subject, discrepancies in the views of the authors before us, still their points of agreement are more numerous than their points of difference, while the former may even be said to be more essential in their character: and it is especially of the solidity of our progress in the knowledge of the minute anatomy of the cord that this may be truly asserted, however manifest it be that there is still vast scope for our inquiries, and that no views hitherto promulgated are so distinctly based as to be entitled to command an universal assent. As to the growth of physiological doctrine, it is, as usual, rash generalization which leads to error in all its departments, and in none more than in this. We can never thoroughly master the wonderful mysteries of that theme which brings us to the confines between an animated and a brute existence, and which would seek to unfold to us the union of both. Yet it is possible that we may still make approaches to it of a nearer description than any which have been hitherto effected, and in directions which have as yet remained unopened, as by lights which it would be presumptuous to foreshadow.

Who shall tell, for example, that it may not yet be shown that the memory is a material garner, in which are stored, as an actual presence, the images it recals? What can be more of a truism than to state, that it is not the eye that sees, or the microscope, or any other optical instrument? These merely transmit the representation which is to be received elsewhere. The eye may be perfect in its structure: but some pressure behind, on the optic nerve or on the sensorium, prevents the conveyance or the perception of the image, and there is no vision; for it is clear that, to produce this, the picture must be carried in its integrity to the point at which it becomes cognisable. The skill of the photographer has rendered us familiar with pictures, in which considerable groups of figures, with their adjuncts, are so inconceivably minute, that they are only visible, yet then distinctly visible, when under a powerful microscope. Who, then, shall say, that sees these triumphs of art, and knows the greater wonders of nature, that memory does not work through the impression of an actual photograph, inscribed and retained within the brain; as if it were but a part of the very limit of our faculties, that we discover nothing in art which has not previously existed in nature? We possess no analogues for similar impressions from other sources; but there is nothing contradictory in the idea that sound, more turbulent in its movements than light, may also, in its own way, impress its phonograph or phonotype

on the brain, and so of the other senses. To recal a scene, then, or a set of features, or a landscape, or a strain of music, may be merely to direct the faculty which first perceived, on that point where the impression was first perceptible, and where it has since remained: and if age bring back the recollections of youth better than those of yesterday, it is but because the undeteriorated apparatus sufficed better, at the one period than at the other, to transmit and preserve the necessary impressions which the mind is otherwise still sound enough to appreciate.

But such speculations, in the present state of our knowledge, may be received almost at pleasure either in jest or in earnest. Even if they could be admitted as ingenious and subtle while they continue in the abstract, they fail ridiculously when we seek their application: and least of all, even could they be carried to a demonstration, are they fitted to be received as furnishing an ultimate explanation, which some have attempted, of the union of mind and body. Between the material and the immaterial world there is, to the human cognizance, an eternity of distance: the fiat of Omnipotence, which has created both, binds them to each other. Nowhere is it easier than here to pass words for realities, and make a new turn of expression assume the dignity of a new truth or a new discovery. But prudent judgments, like those whose labours we have introduced to our readers, will content themselves with the strict field of investigation; and will teach us that the nearer we approach the final difficulty, it is only to perceive it the more distinctly, and consequently to regard it the more humbly. When we stand in wonder before that Ineffable Power which has joined matter to intelligence, and which has made it conscious, not only of self-existence, but of other existences, so as to enable it to act upon these through observation and reason, we shall be ready to own with Tillotson, that a perfect knowledge of nature is nowhere to be found but in the Author of it; and that no less wisdom and understanding than that which made the world, and contrived this vast and regular frame of existence, can thoroughly understand the philosophy of it, and comprehend so vast a design.

Review VII.

On the Nature and Treatment of Club-Foot and analogous Distortions involving the Tibio-Tarsal Articulation. By Bernard E. Brodhurst, Assistant-Surgeon to the Royal Orthopædic Hospital, &c.—*London*, 1856. 8vo. pp. 134.

The activity of the orthopædic surgeons continues unabated; and limited though the sphere of their operations be, there seems as yet no lack of cases to employ them, or of interest attaching to their studies and practice. The curing of club-feet, which was familiar to Hippocrates, but forgotten by his successors, was revived in the last century, and in 1806 had attained the degree of excellence described in the following operation by Sartorius. After he had divided the tendo-Achillis, he says,—(as we are told by Mr. Brodhurst)

"Having fixed the knee, I grasped the foot with both hands, the thumbs being applied to the sole and the fingers to the dorsum of the foot. First, I extended the ankle; then, gradually increasing the force, flexed the foot as much as pos-

sible. Now my assistants held the foot firmly on the table, and I, with all my strength, applied myself suddenly to move the limb forward, which I accomplished, but with such cracking and noise as though all the bones were broken. The patient (a boy of thirteen years of age) screamed terribly: the great pain, however, was soon allayed. On examining the foot, no fracture was found." (p. 10.)

Anchylosis and an useful limb were the result to the patient, and to the profession a stimulus in the right direction, which other surgeons soon followed. Renewed observation and thought developed milder plans of treatment. Delpech of Montpellier conceived the rules for the sub-cutaneous division of tendons, which are substantially those acknowledged in the practice of the present day; Stromeyer proved and made them popular by extensively using them; and he was the occasion of their introduction into England.

"Dieffenbach's account of Dr. Little's return to Berlin, as 'Apostel der Tenotomie,' after having been operated on by Stromeyer, in Hanover, is worthy of its author.

"'A month had elapsed,' writes Dieffenbach, 'since Dr. Little had taken a letter from me to Dr. Stromeyer, in Hanover, when suddenly my door was opened, and the individual who had left me a cripple, entered with a vigorous, rapid step. I cannot tell which was greatest, my astonishment or my joy, but I think the latter. Without delay I examined his foot, and found the shape normal, the sole in contact with the ground, the arch of the foot less; the calf of the foot had begun to be developed, and the entire lower extremity had gained its normal length. A miracle could not have struck me more forcibly; and I must confess that I was never in my life so taken by surprise at the successful result of a surgical operation as by this; and I esteem Stromeyer, who had done it, even luckier than Little, who had been benefited by it.'" (p. 22.)

Since February 26th, 1837, when Dr. Little first divided the tendo-Achillis in England, orthopædy has made great strides. Whilst Velpeau still pursues the violent treatment of Sartorius, Tamplin, Lonsdale, Adams, Coates, Bishop, and Lizars, with other foreign surgeons, have prosecuted the subject; and now we find ourselves occupied with a volume on club-foot from the pen of Mr. Brodhurst, an author already favourably known by his 'Treatise on Lateral Curvature of the Spine.' The present work is only to some extent a new one; for the most part it is composed of papers published by the author in the *Medical Times and Gazette*. After an introduction on the History of the Surgery of Club-foot, the subject-matter of which we have already noticed, and an imposing list of "works referred to," extending over sundry pages, we come to the immediate subject of the book. It is divided into five chapters, of which the first two are devoted to Congenital Talipes, and the third to the distortions commencing after birth. Before the final chapter on the Treatment of the Disease, the subjects of Structural Pathology and Physiology in relation to Structural Shortening of Muscles, and Reunion of Tendons, are separately considered.

The first of these chapters is a compact account of the Physiology, Diagnosis, and Anatomical Pathology of Congenital Talipes; and the descriptions of the author differ in no important respects from those which previous writers have given of the same things. There is no new variety of these deformities to describe, and accordingly the author briefly details the ordinary characters and several degrees of talipes

varus or inverted foot, talipes valgus or eversion, talipes calcaneus, in which the foot is unnaturally flexed, and talipes equinus, in which it is extended, together with the various distortions compounded out of more than one of them. It appears to us important to observe, that all these forms of distortion occur primarily in the muscles and joints of the foot proper, and, until a later stage of the retraction, are entirely independent of the great muscles of the calf. Varus and valgus are affections of the tibiales and peronei muscles respectively, and occur only at the joint which is capable of lateral extension—viz., that between the os calcis and astragalus on the one part, and the cuboid and scaphoid on the other. The former bones do not partake in the deviation; they are as incapable of doing so as they are of eversion and inversion in the normal movements of the foot. The tibio-tarsal joint, or true ankle, on the other hand, admits only of flexion and extension; but here again the excess of muscular action which occasions talipes equinus and calcaneus, appears at first sight confined to the proper muscles of the foot, and the great superficial muscles of the calf, which are inserted into the heel, are only subsequently affected. It is in this later stage that the ligaments are elongated, and the tarsal bones rotated on their axis, while in a still later stage they are compressed into unnatural shapes.

"Rotation of the tarsal bones is in rare instances so great that their replacement becomes exceedingly difficult; yet dislocation does not occur, but rotation on their axis only. An instance of dislocation is, however, recorded, and the specimen is shown in the Strasburg Museum. The astragalus is dislocated inwards and forwards, and displaced transversely, with its posterior surface in contact with the malleolus externus." (p. 33.)

The author's description of the whole of this subject is exceedingly clear, and supplies what one yet feels to be a want in this part of the book—viz., drawings of the skeletons of the deformities which he describes.

The interesting question as to the cause of congenital club-foot, has developed a variety of opinions, which may be classed as follows:—

1. Malformations and displacements of the tarsal bones.
2. Affections of the muscular system.
3. Malposition in utero.
4. Disordered nervous influence.

There is no need to discuss the first of these opinions, for it is plain that bones are not spontaneously misplaced. No better illustration of the adaptation of parts to altered circumstances can be given than in the instance of the bones. It needs but to contrast their overgrown condition in hydrocephalus and the arrest of their growth in hemicephalus, to recognise this rule; but never, except from the absence of a centre of ossification, do we find them the occasion of deformity. No such defect is pretended to exist in club-foot. As to the third opinion, it is sufficient to observe, that deformities are occasionally found in fœtuses at the third or fourth month of gestation, while they are still lying in an abundance of liquor amnii. That the real cause lies in the nervous system, as the author avers, and that the muscles effect the displacements and deformities in question, upon the instance of a faulty nervous influence, cannot be doubted, since those systems have been detected in the act of producing them very soon after birth, at a period

of life not materially differing from that in which the congenital deviations occur. Thus convulsions issuing in slight club-foot, have been produced in a sucking child by emotion in the mother: much more may a similar disease be expected to follow the like cause before the close union of mother and child is broken by parturition. The concurrence of club-foot with various cerebral and spinal diseases is abundantly common, and children who are born with these distortions are frequently subject to convulsive disorders. Moreover, while a fresh cerebral excitement has been known to reproduce distortion after its cure by surgical means, it is interesting to notice that sometimes congenitally-distorted limbs are relaxed, and regain their normal position during the abeyance of nervous influence which occurs in sleep, and under the influence of chloroform. An additional argument might, we think, be drawn from the greater liability of boys than of girls both to cerebral and to spasmodic affections. The statistics of club-foot, in respect to sex, are wanting in Mr. Brodhurst's volume, but he mentions an interesting fact, which is worth much as an illustration of this argument:—

> "Congenital distortions are sometimes hereditary. In 1853, I operated on a child for talipes varus of both feet, who had three brothers, all of whom were born with double varus; and in 1855, a fifth boy was born in this family, also with varus of both feet, who also was under my care, and on whom I operated. There were three girls in this family born intermediately with the boys. None of them, however, were in any degree distorted. The father of these children and his brother were both of them born with double varus, and also their grandfather. In each generation there were females in this family, but amongst them no instance of distortion." (p. 55.)

The whole subject of non-congenital talipes is handled in Chapter IV. After birth many causes of distortion come into operation, besides those which appear to act during uterine life. To the morbid influence of cerebral affections are added various local injuries of the foot itself, the limb, or its nerves; primary, or, as it is called, essential disease of muscle; inflammation of muscle, of integuments, of joints; forced or voluntary mal-position, and debility. Most of these causes of distortion are of course incident to the circumstances of extra-uterine life, and the deformities vary with their cause. We will confine our notice to but one or two of them.

Non-congenital talipes occurs almost always during infancy; and one is inclined to ask, wherein, then, is the practical distinction between the distortions which occur before, and those which come on so soon after, birth? The forms assumed by the foot do not materially differ in the two cases, though a practised eye and hand easily distinguish one from the other. As, however, paralysis is by far the most common cause of acquired talipes, whilst that deformity which exists at birth bears indications of previous spasm, there is necessarily some difference in the mode of treating the two affections. When spasmodic distortions occur in the child (and they are most common in the first three years of life), they form a most suggestive illustration of the mode in which club-foot is produced *in utero*. Some cerebral disturbance usually accompanies the spasm, but often it is transient, and in many instances is not observable. If some irritation in the system can be detected as originating the affection of

the brain, the removal of the cause not unfrequently at once relieves both brain and foot. Occasionally a permanent taliges appears after a slight convulsion, or even without any cerebral symptom having ever been traced or observed. Such a case can only be remedied by surgical treatment.

The paralyses to which the majority of club-feet are due, are distinguishable into three kinds:

1. That arising from organic change in the nervous centre is common to infancy and to adult age. The following case exhibits the occasional connexion of paralysis and spasm,—

"A child, three years of age, met with a severe contusion of the head and fracture of the skull from a blow. In three days the symptoms of concussion had passed away, but those of inflammation ensued. The muscles of the face were spasmodically affected, and the thumb was flexed into the palm. Some few hours later, the hand was clenched, and prone; and, on the following day, the forearm was flexed, the leg was flexed on the thigh, and the thigh upon the trunk, and the extensors of the foot were in a state of clonic spasm. Hemiplegia succeeded. Finally, the spinal column became curved, the concavity being of course towards the paralysed side, and the flexor muscles of the leg and the extensors of the foot became permanently retracted. The fingers also were folded into the palm, the flexor muscles being retracted." (p. 61.)

2. Paralysis may arise from traumatic lesion of nerve-trunks. It is necessarily a rare occasion of distortion, though at the same time it is suggestive of structural change in nerves being a possible cause of deformity in some obscure cases.

3. The third cause of paralysis is disease of the muscles themselves, *myogenic* paralysis, the essential paralysis of infants. In the author's opinion, this obscure affection is probably rheumatic, and is occasioned by too rapid cooling of the body, especially during the recovery from debilitating diseases. Yet he finds the disease limited to one period of early life—from the ninth to the eighteenth month,—preceded by acute sensitiveness of the affected muscles, and sometimes involving single muscles, sometimes associated muscles, sometimes both extremities of one side. When we consider these facts, and the large proportion of male children shown in Dr. West's table to be liable to the disease, we are by no means prepared to acquit the cerebral system of being in some sort the cause. We should draw no argument from Cruveilhier's observation of the atrophy of the anterior roots of the nerves in these cases. Emaciation of the nerves is found in the nerves of the leg in old cases of congenital clubfoot,* but equally with emaciation of the muscles to which they lead. The wasting of the nerves has not been proved to be antecedent to that of the muscles, and the occasional suddenness of the paralytic attack forbids us to expect that it should be so.

The condition of the muscles under the various circumstances in which they are placed in club-foot, constitutes an important study in reference to the treatment of the deformity. Muscles which have for a short time been affected with spasm, exhibit no change of structure; indeed it is long before they present an amount of structural alteration inconsistent with the restoration of their functions. Accordingly, in

* Anatomie Pathologique, tome i. livraison 2.

all recent cases of club-foot arising from spasm, the muscles may, after treatment, resume their action. But as the muscles belonging to an anchylosed joint will waste, shorten, and degenerate, so those which have occasioned permanent displacement of the tarsal bones, and have become inactive, are subsequently altered in structure. Atrophy soon follows inaction, and the structure then undergoes a fatty degeneration; the whole muscle, thus become a slender inextensile cord, forms an insuperable obstacle to the restoration of the foot to its natural position.

"But although the contracted muscles are shortened, their extremities being approximated, they have not at birth undergone structural shortening; nor, indeed, does structural shortening occur until inaction, or some other abnormal condition, has destroyed the power of the muscle: or, if structural shortening is ever found at birth, it is so rare as to prove the rule. That structural shortening has not taken place at birth, is proved by the restoration of the shape of the foot through mechanical means alone. And further, it is proved by the unaided restoration of the limb on cessation of spasmodic action, when the bones do not interfere to prevent the antagonistic action of the muscles, as is witnessed in talipes calcaneus, and in club-hand. In these congenital affections, as spasm ceases to affect one set of muscles, their opponents resume their power, and restore the normal shape of the limb." (p. 92.)

The presence or absence of this so-called structural shortening, is accordingly a question of serious import as to the requisite treatment of the case. If the distorted foot can be moved towards, or replaced in, its natural position by the temporary application of pressure with the hand, or if during sleep, or the anæsthetic action of chloroform, the foot spontaneously resumes its normal shape, it is plain that the offending muscles are neither permanently retracted nor destitute of contractile power; whilst structural shortening must exist, if the shortened muscle prove incapable under any circumstances, of elongation and further shortening. The division of the tendon of the muscle is undertaken with a different purpose in the two cases.

The division of the tendon of a muscle capable of contraction, whether healthy or affected with spasm, is followed by the instant withdrawal of the proximal from the distal end of the tendon, and by the complete cessation of spasm in the muscle so affected. If the limb be kept at rest, the tendon will reunite so perfectly as to leave little trace of its previous division, and the muscle will be found capable of gradual and complete extension to its original length. In this case, therefore, the tenotomy is resorted to for the purpose of removing spasm. If, on the contrary, the tendon be suffered to reunite at its original length in the case of a muscle permanently and structurally shortened, nothing is gained by the operation, the muscle remains inextensile as before. It is indispensable to obtain an elongation of the whole structure at the expense of the new uniting medium; and there seems scarcely any limit to the elongation of which that medium is capable in the early period of its organization. Accordingly, tenotomy is used for a muscle structurally shortened, that it may be lengthened without being stretched. The author supports these views by the results of many experiments which he has made on animals, as well as by the quotation of authorities; and he comments thus in the following passage, upon some observations on the divisions of

tendons, which have recently been published in the *Medical Times and Gazette*:—

"Mr. Adams differs from former experimenters, and states that the space between the divided extremities of the tendon increases from one to two and a half inches. Now this is entirely at variance with what is known to occur, both in man and animals, when the limb is kept at rest, and in a position to favour reunion. But in the experiments undertaken by Mr. Adams, and for which rabbits were chosen, after division of the tendon the animal was allowed to move about, without any protection to prevent motion of the limb. Union was effected, but the uniting medium was stretched and rendered weak. Also in man the same occurs, when extension of the soft material is effected too rapidly. Not unfrequently, in animals, reunion does not take place, if the ends of the tendon are not in some measure approximated; but the ends of the tendon are gradually more widely separated, until the intervening-space may be several inches in length, the lower portion of the limb being drawn downwards by the action of the antagonistic muscles. I have known this to occur in a dog: the intervening space became half a foot in length.

"That the uniting medium may be drawn out even to a greater extent than two and a half inches, is well understood; and in orthopædic surgery this is a most important circumstance, allowing, as it does, of the restoration of a limb to its normal position after structural change in the muscles has taken place, and after the formation of adhesions." (p. 103.)

The last chapter of the work is devoted to the subject of treatment, and principally to the surgical and mechanical management of the various deformities. A few observations are added on the constitutional treatment which is requisite in certain forms of talipes, but they are not intended to be systematic or complete.

Not every case of talipes requires tenotomy, or even mechanical treatment. If dentition, worms in the alimentary canal, and similar sources of irritation, occasion the distortion, that effect vanishes with the removal of its cause. Congenital calcaneus, again, has a tendency to spontaneous cure, and often is cured merely by the naturally greater muscular power at the back than at the front of the leg When the distortion is permanent, much discrimination is needed to appropriate the remedy to the deformity. By a judicious employment of surgical treatment to replace the feet, and the adaptation of suitable instruments along the lower limbs, Mr. Adams has recently restored some power of walking to a patient who was absolutely paralysed below the muscles of the hip. So also in partial paralysis of the muscles of the feet, which appear to constitute the majority of the non-congenital cases of talipes, tenotomy on one side of the foot, and support on the other, will do much to make up for the otherwise irreparable lesion. It should be observed that acquired distortions, whatever their cause, need never be allowed to increase. Mechanical means should be employed to prevent that tendency, even though the cause which gives rise to the distortion be still in action; tenotomy, however, should never be resorted to until the cause has ceased to act.

We need not follow the author through his description of the operations for dividing the several tendons, or of the after-treatment of the various cases, in which we observe nothing new. We are interested to observe, as confirmatory of our opinion, that the muscles of the calf are only secondarily affected, that the author always divides the tendo-Achillis last, sometimes not until the distortion of the foot itself has first been

rectified by treatment, and sometimes not at all. To the suggestion of Dr. Little, which was carried out by Mr. Solly, that in extreme cases of congenital varus the cuboid bone should be removed, Mr. Brodhurst somewhat demurs, not deeming it to be necessary under forty years of age, and only then if other means had failed.

The author makes some judicious observations on the subject of unnecessary tenotomy :—

"In the treatment of distortions, it has been laid down as a law by an Edinburgh authority, that whatever structures are tense must be divided. This statement demands considerable qualification; its implicit observance would lead to fatal mistakes. For instance, in division of the hamstrings, if the knife be not limited to section of the tendons, but is permitted to divide all the structures that are tense, the peroneal nerve will necessarily be incised, together with the fascia and tendons. And it is both unnecessary and hazardous to follow the precepts of Phillips, to divide the retracted muscles in the sole of the foot, as well as the plantar fascia. In cases of old varus, it is of much importance to distinguish between the structures which it is necessary to divide, and those which may be extended mechanically; for if, following the advice above referred to, all the tense and shortened structures were to be divided, nothing would be left in the sole of the foot, and on the inner side of the foot and leg, undivided, but the bones. It is therefore important to determine which are the structures which it is imperative to divide, and to recognise the extensibility of others." (p. 112.)

The author differs from both Mr. Lizars and Dr. Little as to the age for operating on cases of congenital talipes. Mr. Lizars thinks two or three years of age the earliest time at which the division should be attempted. Dr. Little prefers the operation about the age of six or eight months. Mr. Brodhurst, however, considers four or six weeks after birth not too early for operation, if the infant be robust. The earlier the deformity is removed, the easier is the treatment, and the more perfect is the eventual development of the foot. He finds no difficulty in applying the instrument, and no evil consequence from its pressure :—

"The operation, when performed at this time, and the after-treatment, are so simple, that I hold it to be unjustifiable in the surgeon to seek delay, except on other grounds than age alone. The health of the child may require delay; but I know no other reason for postponement, if it be not the convenience of all parties concerned." (p. 114.)

For our own part, we should venture to add one qualification of the author's rule—viz., that the operation should not be practised early in hereditary cases. It is impossible to predicate of any spasmodic talipes that it will not recur; but when there is a known hereditary tendency, it would, we think, be premature, before the period of early childhood is past, to operate for a deformity which is so likely to recur.

The observations in this article apply almost entirely to those cases of distorted feet which by common consent are referred to the care of the orthopædic surgeon. On the subject of distortions arising from disease of joints, we find a passage of which we are not sure that we gather the right meaning. Speaking of articular inflammation as a cause of distortion, the author deprecates the employment of force to effect a reduction of the deformity, so long as any inflammation continues. "The muscles," he says, "become rigid to prevent motion and pain, and as the joint is restored, they likewise are restored to their normal condi-

tion. But when articular inflammation is the cause of muscular retraction, force should never be used to overcome retraction." All this is recognised practice, but when the author adds, "It is preferable to divide, when necessary, every tendon around a joint which interferes with motion, than to risk re-exciting inflammation," we are inclined to say, it would be preferable to let such a case alone. We should be glad to read the author's views on this part of the subject more at large in another edition.

On the whole, we have read the work with satisfaction and profit. That it is well got up, is due to the publisher; that it is well illustrated, must be mainly attributed to the artist; its literary merits belong to Mr. Brodhurst alone.

Review VIII.

1. *Die in und an dem Körper des Lebenden Menschen vorkommenden Parasiten. Ein Lehr- und Handbuch der Diagnose und Behandlung der Thierischen und Pflanzlichen Parasiten des Menschen.* Von Dr. Friedrich Kuechenmeister, Prakt. Arzt in Zittau. Mitglied der k. k. Gesellchaft der Aerzte zu Wien, &c. &c. — *Leipzig*, 1855. pp. 486.

The Parasites occurring in and upon the Living Human Body. A Manual of the Diagnosis and Treatment of the Animal and Vegetable Parasites of Man. By Dr. Friedrich Kuechenmeister.

2. *Fragmentarische Notizen und Abbildungen zur Helminthologie und Parasitenlehre.* Von. Dr. Med. A. Kraemer, Professor Extraord. und Praktischem Arzte zu Goettingen. ('Illustrirte Medizinische Zeitung,' Band iii. Heft 6, mit ii Tafeln.)

Fragmentary Notices of Helminthology and Parasites. By Dr. A. Kraemer. With Plates.

3. *On Cystic Entozoa in the Human Kidney, with an illustrative Case.* By T. H. Barker, M.D., Fellow of the Medical Society of London. 8vo. pp. 18.

4. *On the Trichina Spiralis.* By Dr. Bristowe and Mr. Rainey. ('Transactions of the Pathological Society of London,' vol. v. Session 1853-4.)

5. *Note on Dracunculus in the Island of Bombay.* By H. J. Carter, Esq., Assistant-Surgeon Bombay Medical Establishment. (With a Plate.) ('Transactions of the Medical and Physical Society of Bombay,' No. 2. New Series. Years 1853-4.)

6. *Observations on the Structure and Nature of the Filaria Medinensis, or Guinea Worm.* By George Busk, F.R.S., President of the Microscopical Society. ('Transactions of the Microscopical Society,' vol. ii. p. 65 et seq.)

Of all that great harvest of scientific information which is daily being reaped by the disciples of Bacon, there can hardly be a more welcome sheaf to the physiologist and the physician, than the mass of knowledge

the last few years have afforded us respecting the various forms of parasitic life.

To sum up what has been accomplished in this department of zoology, would be impossible in the limits of an introductory paragraph. Even in respect to their influence upon practical medicine, the results of late researches are too diverse, as well as too important, to be included in a single passing allusion, without risking the charge of flippancy. Something of this kind, however, we must attempt, if only to give the reader the key-note of the following article; the clue by which those who rightly appreciate the dignity of the art of healing, will trace out the details of the various observations and dissections recorded in the works we shall allude to.

But a short time since, and medicine scarcely recognised any save the human entozoa. And although it did not claim to understand much about even these, still, by a curious kind of fatality, the little it did venture to affirm respecting them was almost uniformly wrong. It supposed them to arise by spontaneous generation; a process in which a peculiar cachexia of the person they inhabited formed an essential (if not the chief) part. The innumerable horde of symptoms they were alleged to produce, included many of the vaguest and most trivial description. Their whole function of nutrition was a mystery: its structures were generally misinterpreted; the substances it acted upon often doubtful; its details always completely unknown. Their reproductive organs prepared what were obviously the germs of an offspring; and what therefore, as such, almost disproved the theory of their own spontaneous origin. But not even the boldest and most skilful conjectures could follow these germs to their maturity. Lastly, the medical treatment that was directed toward the removal of these entozoa paralleled the obscurity of what was alleged about their life and habits. The constitution of the patient was to be strengthened, so as to shake off the cachexia to which these creatures owed their origin. Or conversely, it was to be poisoned, so as to induce the wretched entozoon to emigrate in sickness or disgust. Or, by a judicious selection of the poisonous drug, or a dexterous adjustment of its dose, the parasite was to be slain, while its living domicile remained unhurt;—a kind of perilous reversal of the juggler's feat of breaking an iron anvil on a person's stomach. And finally, even where the parasite inhabited the intestinal canal, and was therefore really amenable to agents introduced into this cavity, the physician and the quack often occupied much the same footing. Not only did the knowledge of the former scarcely surpass that of the latter, but even the treatment of both was alike empirical. They did but vie with each other in the quantity and virulence of the purgatives they prescribed. Indeed, on the whole, the worm-doctor often had the advantage. In rare instances, it is only fair to suppose that his longer experience, or his better choice of remedies, might afford him a real superiority over his orthodox competitor. But, in the vast majority of cases, his inferiority itself placed him on a vantage ground. For inasmuch as he knew less, he ventured more; and would scarcely think any risk or suffering too great for his patient to go through, provided only he himself could add another specimen to the scores of bottled victims by which he displayed his success on his well-known stall in some country fair or market.

Now, however, we seem to be rapidly approaching an era in our knowledge of these creatures, when every one of the above propositions respecting them must be exchanged for its logical contradictory. The theory of their spontaneous generation has long died out, though retained (we may almost say, *embalmed*) in some medical treatises of a much later period. The symptoms of their presence have been weeded of many of the trivial details formerly included amongst them. Their treatment is probably so far simplified, that no practitioner of respectable skill would now think of salivating a patient for cysts of the liver, or dosing him with camphor to poison a Guinea-worm in his leg. The cachexia alleged to be necessary for their origin, is known to be often wanting; and even when it exists, to be the effect, rather than the cause, of their presence. Last, but not least, their development has in many instances been traced with sufficient success to add a new and interesting chapter to the physiology of the generative function; to explain the way in which the parasite gains access to those structures of its victim which it is destined to inhabit; and, in many cases, to afford a prophylaxis that will ultimately go far to compensate for the limited efficacy of the still empirical (though now intelligent) treatment which is directed specially to their removal from the body.

The work which figures at the head of our list, does indeed denote something like an epoch in the history of the anatomy of these creatures. Since the time of Bremser, no treatise that we know of has ever laid before the public so large a mass of information as that it furnishes. And it is but justice to the author to add, that he is no mere compiler. On the contrary, much as has been undoubtedly contributed by other inquirers—much as we owe to Steenstrup, Eschricht, van Beneden, Siebold, Kölliker, Dujardin, Diesing, Owen, Busk, Rainey, and many others—Küchenmeister has himself added so large a number of observations and experiments, and these form, in some respects, so fitting a climax to the labours that have preceded them, that we may congratulate ourselves on having (slightly to alter a cant phrase of the day) the right man for the right book.

Almost the first paragraph of the work contains a statement which, were it not that Peter Pindar's laughable doggrel is too untranslateable to have ever experienced the honours of a German edition, seems expressly levelled at that immortal couplet which records the existing belief of the naturalists of his time, and states that

> "These fleas have other fleas to bite 'em,
> And these fleas, fleas, *ad infinitum*."

"So far as is hitherto known," says our author, "the parasites that infest the human species are *not* troubled with parasites themselves." Hence, sad as it is to lose so good a metaphor, we trust the British orator will give up all further citation of these lines; and concede to this stock quotation the repose so much needed by it, the Phœnix, the Upas-tree, the gardens of the Hesperides, and the foot-notes of the Eton Latin Grammar in general.

The *Helminthoid* class includes the three chief groups of Cestoid, Nematoid, and Trematoid worms: the affinity of which is evinced by the close analogy of their structure. In all three, the special senses are absent;

the integument is a softish chitinous envelope, more or less minutely wrinkled in squares; the (unstriped) muscular tissue is arranged in longitudinal and transverse bands, and consists of a delicate pulpy sarcode that sweats through the integument on the continued application of water, and collects in drops on its exterior; the canals that enclose and convey their nutritious fluids are generally multiple, often elaborately branched; and lastly, their development generally includes a stage of active or passive migration, during which the animal occupies a different habitation, and possesses a simpler and lower grade of structure, than those it respectively assumes in order to reach its maturity.

The *Cestoid* group is the first to claim our notice. Let us trace the history of an ordinary tape-worm or Tænia, *ab ovo.* The joints of the adult creature seem, in some species, to undergo a disintegration within the intestine of the animal they inhabit. Thus Küchenmeister (p. 10) on one occasion found the wall of the large intestine of a dog occupied by a white, sandy powder, the particles of which, on examination under the microscope, turned out to be innumerable ova of a *tænia serrata* higher up in the bowel, and were accompanied by separated joints of the animal. Hence it would appear not impossible that such liberated ova may sometimes experience the next stage of their development within the body of the animal to which the parent is attached. And in one or two instances the connexion of the *Cysticercus cellulosæ* in the muscles, with the previous presence of *Tænia solium* in the intestinal canal, has been verified in the human subject. A systematic inquiry will perhaps reveal numerous cases of this kind; and show that this contingency constitutes one of the most serious dangers which the mature parasite inflicts on the animal it inhabits, and one of the strongest indications for its removal.

But in a majority of instances it would seem that the joints (*proglottides**) of the worm are discharged from the body either singly or in numbers, still retaining an active vitality during a short period. At least it is difficult to conceive this expression as exaggerated, when we notice (what some of our readers have perhaps done) the violent contractions they offer shortly after their expulsion. The long single joint thus expelled often exhibits what seems to be an alternate contraction of the longitudinal and transverse fibres, which further engages the two sides of the segment in varying degree at different times. The result of this sequence of contractions is to produce movements which, supposing the disengaged joint to be lying on the ground, almost simulate† those of progression in a vermiform animal; and are, at any rate, capable of moving it to some little distance from the spot on which it may have

* It is really necessary to protest against some of the Latin and Greek terms which are being gradually imported into the language of science, as research multiplies objects of observation, and demands new names for them. The term *proglottis* is now made use of to denote the severed joint of a mature tape-worm filled with eggs or pupæ, or the analogous structures of some other entozoa. If this kind of nomenclature goes on, no student will, by and bye, be qualified to attend medical lectures or read medical books, without a year or two's preliminary study of glossaries.

† Küchenmeister describes such joints as "discharging eggs during their march along the ground"—an expression which is physiologically incorrect, and certainly exaggerates anything the Reviewer has ever seen of such movements under the most favourable circumstances (for example, in vivisections of animals).

fallen. The same contraction has been seen by Dujardin to expel some of the ova from the interior of the segment.

The structure of these ova (as they are called) exhibits far too marked a differentiation to entitle them strictly to such a term. It is only in the earlier stages of their development, if even then, that they are really the analogues of ordinary ova. As regards their contents, the possession of the booklets renders these contents an embryo, rather than an ovum. As to their shell, it has seemed to the writer of this article, that even in the blind extremities of the branched canal which constitutes the oviduct, this is a distinctly calcareous substance, composed chiefly of carbonate of lime. And by the time they reach the central segments of this tube, their transparent shell is not only much thickened, but is converted into a dark yellow, or rather brown mass, by the interstitial deposit of a chitinous substance. At the same period, if not earlier, the hooklets of the enclosed embryo may readily be detected, without any preparation, by a moderately good microscope, especially with the aid of an achromatic condenser. The admixture of these organic elements with the calcareous shell, imparts to the latter that extraordinary power of resistance to chemical, and even mechanical violence, which it certainly possesses. The striæ which seem to pass perpendicularly to the surface of the shell throughout its whole mass, appear on careful examination to correspond with radii from the centre of the ovoid embryo; and to be, therefore, not quite parallel to each other. From the effects of carefully shifting the light that falls upon them, there is some little doubt whether they may not be due to circumstances of refraction, rather than to any really fibroid or tubular arrangement of definite structure. The capacity of resistance just alluded to, is an important point for consideration in connexion with the possibilities of the transmission of the embryo in time and space. The dilute acids and alkalies have little immediate effect on this leathery husk: and even after having been applied to it for hours, scarcely effect more than a slight swelling and transparency.

How far they may affect the life of the embryo within this dense and horny case, it is difficult to say: though from analogy we can hardly doubt that many chemical reagents can destroy its capacity of development, just as even an external deposit might perhaps have the same effect by preventing all interchange of gases, or any entry of oxygen from without. But what between their mechanical toughness and their chemical resistance, these pupa-sacs (as Mr. Busk, we believe, proposes to call them) defy, to all appearance, most sources of injury or decomposition. After months of exposure to warmth and moisture, the pulpy and putrid *débris* of segments of the *Tænia solium* yield ova which show no sign of any approach to degeneration or decay. And the writer of this Review has been struck by the remarkable way in which the size and structure of these ova sometimes allow them to elude all precautions that may be taken against their mechanical dispersion. In spite of every attempt to ensure their destruction, by steeping the specimen glasses he may have used in strong acids, and by afterwards bathing them in the flame of a spirit-lamp, he has once or twice found the characteristic ova appear most unaccountably in healthy and diseased tissues or secretions of the human body which he has subsequently examined with these glasses.

The speedy death of the expelled joints is followed by their putrefaction; a process which is, of course, hastened by warmth and moisture. And the dissolution of the parent tissues ultimately sets free the eggs contained in their interior, to be carried by the winds or waves wherever accident may determine. How vast a number of them miscarry, is evident when we attempt to take the census of a single tape-worm; or imagine the millions of eggs such a parent foists upon society during the years it may inhabit a given animal. What becomes of these abortive germs, how long they retain any vitality, and what are the circumstances that may rob them of it,—are questions we cannot answer, save by the conjecture that their albuminous and fatty materials are either applied to the soil in a decomposed form, or are consumed as food by various of the minute invertebrata that throng the surface of the earth and the waters. But the more fortunate minority of these eggs, the destiny of which is to eat instead of being eaten, after many and long wanderings of this passive nature, are at length engulfed by some unconscious animal in company with its food; and, through its alimentary canal, attain the locality of their second form of existence.

During this passive migration the ovum has retained its previous size ($\frac{1}{700}$th in.) and shape. But its thick wall now bursts, and sets free the enclosed embryo, which is an ovoid body, of nearly equal size, armed with six hooklets at one extremity. Impelled by instinct to begin its active migration, the embryo pierces the first portion of its path by bringing together the anterior pair of hooks so as to form with them a kind of wedge-shaped stiletto; and now drags itself forwards in the same direction by means of the two succeeding pairs of hooks, which it uses (to adopt the simile of our author) like a person who, in attempting to get out of a bow window, thrusts himself forwards by his elbows. In this way the minute embryo penetrates the body it inhabits, and only ceases its efforts on reaching the place its instinct recognises as suitable for its abode prior to the next series of changes it has to undergo. Streaks of reactive inflammation and exsudation generally indicate the minute channel by which the embryo thus traverses the wall of the digestive canal in its course to the liver, or other organs.

The migration by which the hooked embryos of the *Tænia solium* or the *Tænia cœnurus* traverse the body of the animal they inhabit, is thus suggested to be an active one:—a true locomotion, effected under the impulse of an instinct, and by means of certain special organs. But we must not hastily prejudge this important question by assuming that their migration is exclusively of this kind. On the contrary, in the present stage of our knowledge we may preferably bear in mind that there are fair grounds for conjecturing that the process is partly a passive one, at least in some of these parasites. Such a view is indeed suggested by the comparative rarity of the streaks of reactive inflammation alluded to —which ought, on the theory of an active migration, to be as numerous as the *Cysticerci* or *Cœnuri*, and as long as the interval between their site and their starting-point in the intestinal canal. And the channel of this passive migration is equally obvious. Presuming that the hooked embryo had once penetrated a vessel, it could hardly fail to be swept away by the rapid current of blood, and thus carried onwards, until arrested by its own

attachment to the walls of the vascular system, or by its impaction in the narrowing calibre of a particular set of vessels. The numerous modifications which might be impressed on such a process by the size of the embryo, its choice of nutriment, its vigour of movement, we dare not attempt to indicate. It may suffice to add that the numerous (and therefore authentic) cases in which entozoa have been found in the blood, are completed by a communication from Leuckart to Küchenmeister, in which he states that he has found the embryos of Cestoid worms in the blood; and in such numbers, that he inclines to regard this as the ordinary channel of their migration, and the clue to the wide diffusion of their *scolices* throughout the body.

According to Küchenmeister, this intermediate stage of life seems to vary greatly in different species of Tænia. The embryo of one species, for example, penetrates the liver, where, by a true alternation of generation, it is converted into a cyst, from the interior of which are developed the heads (*scolices*) of the future tænia (*Echinococcus veterinorum seu scoliciparieus*). An equally definite alternation probably occurs in another species, in which the similar cyst developes secondary and tertiary cysts, prior to the formation of scolices within their interior (*Echinococcus hominis seu altriciparieus*). In both, the cysts may fail to execute the last act of development, and remain barren of scolices.

The *Cysticercus* and *Cœnurus* also appear to represent a stage of development that necessarily implies the growth of new embryos within and from the original one. In the animal inhabited by the *Cysticercus cellulosæ*, the muscles, the areolar tissue, the brain, or the eye, are occupied by a variable number of larvæ thus produced from the embryos of the *tænia solium*. The *Cœnurus cerebralis* that so frequently inhabits the brain of the sheep, is similarly produced from the embryo of a *tænia cœnurus;* with the difference, that instead of the embryo enlarging after the shedding of its hooks, and developing a single scolex from a granular thickening of its interior, it produces a number of scolices (800 or more).

The larvæ thus formed only complete their development into their corresponding species of Tænia on being introduced into the alimentary canal of another animal. And as some of them—such as the cysticercus—occupy situations in which they are not necessarily fatal, a vast majority never experience this development at all, but degenerate and decay in the animal they inhabit, on reaching the term of their existence. Others—as the Cœnurus—almost ensure their liberation from the tissues they temporarily inhabit, by causing the death of their host. But even of these, a large proportion necessarily miscarries: indeed, putrefaction of the surrounding flesh appears always to kill them in a very short time. The experimental proof of these propositions it is one of Küchenmeister's chief merits to have established. These experiments, some of which date as far back as 1851, have been since repeated and confirmed by many other observers: especially by Von Siebold.

The conversion of the six-hooked embryo contained in the ovum of the tænia solium into a *Cysticercus cellulosa*, may be effected by feeding swine with the joints of the tape-worm mixed in its food. In seven weeks from the time of first feeding three swine with these eggs, he found one of them to contain Cysticerci, the vesicles of which, the size of a hempseed,

began to exhibit a central cloudiness. A fortnight later, of the Cysticerci contained in a second swine, the largest individuals had attained the size of a pea; and their heads were beginning to be distinct. A fortnight later still, a third pig was occupied throughout its whole body by Cysticerci of different size and maturity.

The conversion of the Cysticercus into the Tænia, or of the scolex into the mature worm, has been similarly effected in this animal. But our author adduces a much more striking example of the same fact, having succeeded in feeding a condemned criminal with the Cysticercus, and verifying the presence of the tænia in his intestine after execution. The criminal (condemned for murder) was fed with Cysticerci in numbers of 12, 18, 15, 12, and 18, at five corresponding meals, 72, 60, 36, 24, and 12 hours before death. They appear to have been partly disguised by their resemblance to the grains of rice in warm rice-soup, partly by their likeness to the small bits of paste in a kind of vermicelli soup; and partly foisted on the unhappy wretch by being substituted for the small lumps of fat in blood-puddings.* The Cysticerci had all lain 72 hours in a cellar before being thus devoured by him; and hence some of them had been 130 hours outside a living organism. The necropsy, made 48 hours after death, revealed ten young tæniæ attached to the intestine by their hooks and suckers. Their length was $\frac{2}{3}$—$\frac{1}{3}$ inch; and they had an appendix of half this length, depressed or inverted, like that seen in the Tænia of the dog's intestine three days after being fed with the Cysticercus of the rabbit.

The production of the cœnurus from the eggs of a corresponding Tænia (*Tænia cœnurus*), and again of this tape-worm from the Cœnurus, precisely repeats the more material points of the preceding statements. The course of the author's experiments, however, reversed the above order.

He first fed a dog with the *Cœnurus cerebralis* obtained from a sheep; and on killing him two months after, detected the above tape-worm. The very same day, the joints of this *Tænia cœnurus* were administered to a sheep. Fifteen days after, the latter animal began to show signs of the rotatory disease; and in three days more was so "stupid," that it had to be killed. The necropsy showed the surface and the third ventricle of the brain occupied by fifteen vesicles of about the size of a hempseed: the cerebral surface and substance in their vicinity being traversed by yellow streaks of exsudation, like the burrow of an itch-mite.

Our space obliges us to sum up a host of negative experiments performed by Küchenmeister and others, as leading chiefly to the following conclusions. Each species of tænia has its own definite cysticercus:—the ova of the *Tænia solium* produce none but the *Cysticercus cellulosæ;* those of the *Tænia serrata*, the *Cysticercus pisiformis;* those of another tænia, the *Cysticercus tenuicollis;* those of the *Tænia cœnurus*, none but the *Cœnurus.* And these experiments rarely succeed in more than one or

* What does our English reader think of the moral side of this experiment? The Reviewer is aware that much may be said in favour of using these and similar opportunities for the promotion of science. But he protests against a living fellow-creature being regarded in the light of a mere subject of experiments of this kind, even though he be a murderer whose hours are numbered. And he ventures to think that few would controvert the conclusion of one of the most eminent physiologists of the day, who indignantly alluded to this experiment as being "debasing to our common nature."

two species of the domestic animals:—the pig, for example, is the only animal (beside man) in whom the *Cysticercus cellulosa* has been thus produced: the *Cysticercus tenuicollis* is limited to the goat and sheep; the *C. pisiformis* to the rabbit; the *cœnurus* to the sheep. In other words, it would seem that most, if not all, of these parasites are so far limited to one or two species of mammalia, that all others enjoy an absolute immunity from their attacks. It even appears likely that the animal infested by a given parasite at one stage of its development, may be altogether free from the presence of the same species at a later stage of its evolution; may be infested, for example, by a given Cysticercus, but not obnoxious to its subsequent Tænia.

The importance of such facts it is not easy to overrate. If hereafter confirmed (and many and careful must be the experiments that alone can establish them), they will add another and remarkable illustration of the way in which Nature antagonizes an extreme fecundity of the ova of these entozoa, by the parsimony with which it supplies the conditions of all further development. From the very mode of their introduction into the body, eggs and larvæ must alike be scattered in millions, before one of them can light upon its future dwelling-place. And even this numerical disproportion is increased as we trace it into further details. The larvæ are sometimes imprisoned during their whole life in the animal they infest. They are often destroyed by its death and putrefaction. Thy are probably the prey of many small animals, whose alimentary juices kill and dissolve them. But one or two species of higher animals are qualified to serve as their habitations, or are fitted to nourish their organism with the delicate chylous fluid which they appear to demand as their food. And lastly, even in the particular species they affect, a casual diarrhœa may prevent their attachment to the alimentary canal; just as an artificial one can remove them from this tube, after many years of uninterrupted fixation to its walls.

Of the two species of *Echinococcus* that infest the human subject, the *scoliciparìens* has been traced by Von Siebold and Küchenmeister into a tænia that inhabits the intestine of the dog. The *altricipariens*, in which a secondary cell-growth is interposed between the enlarged embryonic cell and the larvæ or scolices, has not yet been successfully traced to maturity: but it may be conjectured to produce a Tænia that infests man as well as some of the domestic mammalia.

The presence of this Echinococcus is so frequent in Iceland as to constitute a dangerous endemic disease. Schleipner and Thorstensen fully confirm each other as to its extraordinary frequency. The former saw 57 persons suffering from this malady during his stay in the island; he regards it as far more common inland than on the coast; and estimates that in some of the worst districts it affected two or three individuals of every family. Further, his estimate, that it formed one-eighth of the total cases of disease, tolerably corresponds with that of Thorstensen, who calculated that it affected one in seven of the whole population.

Our author devotes a few pages to the discussion of this endemic; not, however, with much success. He notices the prevalence of vertigo among the cattle (suggestive of Cœnurus); tells us the size of the sheep, the

amount of their wool, the species of the dogs used to assist in the pastoral care of these animals, as well as of the equally numerous oxen. He alludes (with more significance, we think) to the remarkably even temperature of the seasons; to the warmth and moisture of the air caused by the hot-springs; and to the comparative deficiency of vegetables. Finally, he points out the carelessness of the butchers, the rustic simplicity with which the population pay the rites of Cloacina—or rather altogether ignore her fane; and the extreme uncleanliness of their dung-heaps. The suggestions he offers mingle the zeal of the discoverer and the art of the physician just as irregularly and confusedly as the above details. Indeed, if we may say anything so invidious, this chapter offers us that exquisite disorder in order which seems to characterize, not so much the German philosophy, as the logical forms and the etymological structure of the German language itself.

Deferring all consideration of the prophylaxis that belongs equally to these and other entozoa, we would just point out that many of these circumstances may be at once eliminated from any inquiry into the causation of the epidemic. Dung-heaps are dirty, butchers careless, and water-closets scarce, in many other parts of the world besides Iceland. The influence of warmth and moisture would probably favour the production of more entozoa than one single species. And even granting that the comparative rarity of the swine in this island explains the comparative rarity of the Tape-worm, it does not seem at all made out that the dozen other species of Tænia elsewhere inhabiting the domestic mammalia, are unusually frequent here. The Cœnurus, even if often present, would not explain the Echinococcus, or more than partially answer this causative requirement. Indeed, the first and most specific part of the inquiry is evidently that of determining what is the species of Tænia amongst these mammals that corresponds to the human Echinococcus: a question that if it cannot be answered directly by feeding any of these animals with Echinococci, might perhaps be indirectly replied to by detecting some species with disproportionate frequency in their alimentary canal. That such Tænia, whatever its species, is not often developed in the intestine of the person infested with the Echinococcus, is a conclusion that is almost forced upon us by the presumable infrequency of all Tape-worms amongst these islanders. It would therefore almost appear as if human beings, though amenable to the ravages of the scolex, did not afford the suitable habitation for the mature worm into which the Echinococcus is developed.

Our author devotes a few pages of his Appendix to a consideration of some observations by Zeller and Virchow which would claim for one or two cases of alleged "alveolar colloid," of the liver, the real import of Echinococci in this organ. The grounds for this diagnosis are so simple that it is not necessary to lay much stress upon them; the detection of the parasite being one which of course negatives every other view. Indeed, Küchenmeister states that he had been so long aware of the chief peculiarities of this state in the lower animals, that he had passed over it in a few words; though he fully recognises the importance of Virchow's remarks in relation to human pathology.

The chief peculiarities of this state of liver refer to the size and arrange-

ment of the cysts. They are mostly small, of a size varying from a millet-seed to a pea. They occupy the interstices or alveoli of a striated and apparently fibrous tissue; the cavities of these neighbouring alveoli communicating with each other by orifices of variable size. In respect to their contents they differ, not only in the fact that some (especially those on the surface of the liver) are barren of all traces of the cast-off hooks; but also in the degree of degeneration of the parasite and the hepatic fluids which their gelatinous mass exhibits. Lastly, as regards their arrangement, they take a course in the direction of the biliary vessels towards the intestine, in the neighbourhood of which they seem to be younger, more fertile, and less degenerated. Hence he regards Virchow's suggestion—that they are in some way the common offspring of a single immigrant, and that they extend mainly along the lymphatics of the organ—as probably correct. Indeed, he suggests that they are in point of fact processes of a single cyst, radiating from it as a centre along these vessels, to be then constricted and shut off from the central parent sac;—processes which are therefore fertile or otherwise, according as that part of this sac from which they are constructed had or had not begun to develop scolices at the time of their isolation. The theory is ingenious; and though open to some serious objections, evidently combines more of the facts than any other yet offered.

How far the tribe of Helminthia, so sparingly represented in man by the *Bothriocephalus*, offers any analogy to the above Tænia in effecting an active migration, remains at present very doubtful. Küchenmeister inclines to decide this question in the negative, in all those species which are apparently devoid of the hooklets that evidently form the organs of this locomotion in the embryo Tænia. Where the embryo Bothriocephalus possesses these hooklets, he thinks such a migration may possibly obtain. But in those species in which the embryo and the mature worm resemble each other in the absence of these hooks, he suggests that the migration is never more than a passive transference from the intestine of one animal to that of another. Indeed, in some cases it would seem that there may be no migration at all; that the ovum, extruded from the parent worm in the intestine of the animal it inhabits, then and there allows the escape of an embryo; which, even at this early date of existence, possesses the bothridia and the central bulb that distinguish the adult. But in the absence of such a resemblance, this mode of development is at most an unlikely one.

The passive migration, however, seems to be more frequent in these Bothriocephali. This migration seems to be generally from the intestine of a lower animal to that of a higher one; the carnivorous habits of the latter being made instrumental to its own reception of the worm. Thus, Creplin's observations, according to which the asexual and immature worm contained in one fish, becomes converted into a mature Bothriocephalus in the intestine of another fish, or of some bird of prey that has devoured and digested the body of its former habitation, represent a frequent occurrence in the development of this group of the cestoid entozoa. Even here, however, we are left in doubt as to whether there may not have been a previous stage, in the body of some other animal, in which the ova underwent a development to the degree of scolices, prior to being devoured by the fish that conducted them to their immature state. Hence, applying all this to the *Bothriocephalus latus* that infests the

human subject, it is evidently impossible to say how this worm is first developed, where its scolex or larva is formed, and by what means (or even in what form) it is introduced into the intestinal canal. As regards its geographical distribution, Küchenmeister inclines to connect it with the migrations of the Tartar tribes, and to the moist character of the countries in which this worm occurs; in both cases, we need scarcely say, without any attempt at a definite conclusion.

The remarkable prevalence of the *Bothriocephalus latus* on certain of the northern coasts of Europe, seems to place its development in some definite relation to the fish-eating habits of the people who inhabit those districts. And though this entozoon certainly occurs in inland countries —such as Switzerland—to which any conjecture of this kind will hardly apply, still the liability seems never to approach the maximum stated for some sea-coasts. At least it is difficult to avoid deducing this conclusion from statements like that of Huss and others, as to the distribution of an endemic disease of this kind in the above localities. Huss (Krankheiten der Schweden) describes the Tænia lata (presumably the *Bothriocephalus latus*) as extremely common on part of the Lapland frontier (Norbattin) in Finland, and on the shores of the Gulf of Bothnia.* On the coast itself, there is scarcely a family altogether free from it—old and young, rich and poor, natives and immigrants, alike suffer from the worm. Indeed, even in one or two large towns on the mouths of rivers, at least two per cent. of the whole population experience its attacks. On passing inland, the frequency of the disease diminishes, until, eight or nine leagues from the coast, it almost ceases to be found. It is very interesting to notice that, in curiously direct contradiction to these striking facts, the natives themselves believe it to be hereditary. Huss himself seems to incline towards the more reasonable theory that attributes it chiefly to the diet, to the milk and fish—especially the salmon—that form so large a staple of the food in these districts. While he is careful to point out that the mountaineers inland are almost devoid of it, in spite of a diet almost exclusively carnivorous.

The section devoted to the *Trematoid* worms does not afford so much that is new to the pathologist. The admirable researches of Steenstrup, Von Siebold, and others, on the alternation of generations that prevails in many of the *Distomata*, have been so many years before the English public in the shape of Mr. Busk's 'Translation of Steenstrup's Memoir' (Ray Society, 1845), that we are spared the necessity of further allusion to their details. Our author ventures to conjecture that the flukes which infest the sheep, are derived from the *Cercariæ* contained in the various snails of the low marshy pastures in which the affected flocks are fed. But the stages by which the egg of the *Distoma* undergoes its development into the *Cercaria* remain still unknown; or rather, though we have some analogical grounds for conjecturing the nature of these changes, we are quite ignorant of the animals in which the embryo resides while going through them, and the degree in which each change of residence furthers their advance to maturity.

* The comparative absence of saline content in these waters is interesting in connexion with the prevalence of the worm in inland lake districts of Europe. It perhaps points to a fresh-water fish as the source of its introduction to the human body, or at least to a fresh-water marine animal, as its dwelling during one of the previous stages of evolution.

The comparative rarity of the *Distoma hepaticum* and *lanceolatum* in the human subject renders them less interesting to the physician than many other entozoa. The separation of the two species is rightly insisted on by our author.

Three other species are added—the *D. heterophyes*, *hæmatobium*, and *ophthalmobium*—on the authority of Bilharz, Griesinger, and Diesing. Of these three *Distomata*, the first has been noticed twice, in large numbers, in the small intestine of an Egyptian; the last once in the eye of a child five months old, at death. The remaining species, *D. hæmatobium*, is especially interesting, alike from its frequency in some parts of Egypt (according to Griesinger, 117 times in 363 necropsies, equal to 33 per cent.), and from the grave and characteristic symptoms and appearances to which it often gives rise.

The difficulty that Bilharz and Griesinger have both found in determining the nature (ovum or larva) of the embryo within the body of the animal inhabited by the parents (the *Distoma* being bisexual, the female often contained in a canal of the male), is one that it seems impossible at present to clear up. The *Distomata* inhabit the venæ portæ and its branches, the intestinal canal, the walls of the urinary bladder, the ureters, or even the pelvis of the kidney. In the liver they seem to do less mischief than in the urinary apparatus: though the choking-up of the portal trunk with adult *Distomata*, and the deposit of eggs in the substance of the liver itself, which they bring about, must necessarily derange the function of this important organ, may starve it of blood, or perhaps irritate it to abscess. In the intestine they are often associated with appearances resembling those of dysentery:—with congestion, extravasation of blood, deposit upon and beneath the mucous membrane, fungoid excrescences, and croupy exsudations that occupy ulcerated patches of the bowel. In many of these cases, the eggs of the creature may be found wedged in long rows within the intestinal vessels,* or in and beneath such exsudations, or on the free surface of the mucous membrane. Hence Bilharz had suspected whether the dysentery endemic to Egypt might not have to the presence of these distomæ the same relation as the itch has to the *acarus*. But both he and Griesinger (says our author) convinced themselves that the coincidence was accidental; that there was no causal relation between the dysenteric appearances and the *Distomata*, since in many such diarrhœæ the parasite could not be detected.

Is not this decision rather too impartial? Or rather, is not this an impartiality somewhat akin to carelessness? No one can suppose that *all* dysentery is due to the *Distoma*, either in Egypt or any other country; just as no one has any right to presume that *all* cutaneous irritation, even in the Highlands, is of that kind for which sulphur used to be thought the only specific. But surely, when we consider what must be the physical effects of the presence of the *Distoma*, and what lesions it must inevitably produce, we can hardly doubt that such damage to one of the most vascular and inflammable tissues of the body, must often be followed by changes that (from their nature and locality) will almost necessarily simulate dysentery.

Such a conclusion receives a strong confirmation when we turn to the

* A further confirmation of the passive migration of entozoa by means of such channels, alluded to at p. 118.

lesions produced in the urinary apparatus. Here the mucous membrane appears swollen in places which are covered with a soft, sandy, rotten mass, that is firmly fixed to the subjacent tissue. The microscope shows this mass to consist of the full and empty shells of the parasitic ova, imbedded in a mixture of blood, exsudation, modified epithelium, and crystals of uric acid. The thickening of the submucous tissue often produces stricture of the ureter, which is followed by retention of urine, and all its dangerous consequences :—degeneration of the kidneys, pyelitis, dilatation of the pelvis, or atrophy of the renal substance : or the masses themselves become the nuclei of calculous deposits, and thus aid in the chlorotic exhaustion these creatures produce in the person they inhabit, by the consumption of blood they imply. Lastly, it seems not unlikely that the dislodgment of clots into the general circulation sometimes brings about pneumonia, in the way described by Virchow, and illustrated by the clinical researches of Kirkes.

Amongst the *Nematoid* worms, Küchenmeister first describes the *Trichocephalus dispar*, that haunts the large intestine and end of the ileum of the human subject. According to Rudolphi, this entozoon is extremely common : and the fact that it is rarely detected, must be ascribed chiefly to its colour and contents rendering it so like the fæcal mass with which it is mixed, as to allow it to escape notice, unless the latter be carefully washed and sifted so as to collect the contained entozoa. Such a mode of examination also shows that they are generally present in very large numbers.

The well-known details of their anatomy receive little addition from Küchenmeister's researches. Their mode of attachment to the intestine—some kind of fixation being, one would think, indispensable to their sojourn in the canal—also remains still unknown.

It is chiefly with respect to their development that Küchenmeister comes forward as a discoverer. He regards it as very probable that the *Trichina spiralis* and the *Trichocephalus dispar* have to each other the same relation as that which his observations would deduce for the *Tænia solium* and the *Cysticercus cellulosæ*. To speak more exactly, he suggests that just as the myriads of Cysticerci disseminated through the muscular system of a pig, represent the devoured larvæ of tape-worms, so the innumerable Trichinæ found in the muscles of man and certain mammalia are the larvæ of the Trichocephalus that inhabit the intestine.

Such a theory can of course only be based upon facts as definite as those which Küchenmeister brings forward in the case of the Cestoid worms. And in the absence of these, but little stress can be laid upon analogies to a different class of entozoa :—a class in whom many of the chief conditions of development appear to vary so remarkably, even in kindred genera.

The weak point in our author's theory therefore is, that he has hitherto fed animals with flesh containing Trichinæ, without producing any such results. And Leuckart seems to have been scarcely more successful; save in a single experiment, in which Trichinæ were found in the intestines of mice who had been fed thus two days before.* But it is obvious that

* Unhappily, the remaining mice of the group had devoured each other (imitating the vices of their tyrants, as exemplified by the cats of Kilkenny). The zeal of these converts to carnivorous food (like that of their human antitheses, the vegetarians) appears to lead them into indiscretions.

such an experiment will not support any conclusion whatever. While the apparently conclusive results of Herbst* (in which the trichinous flesh of a badger had disseminated the same parasite through the flesh of three puppies in three months after) are not only opposed by the failures of the above observers to reproduce his results, but seem quite at variance with any phenomena of migration hitherto known to us. For the entire cyst can hardly be capable of any transit from the alimentary canal to the muscles, whether active or passive. And the Trichina it encloses not being a parent, or even a nursing individual, can scarcely be supposed to reach the muscles for a mere re-imprisonment—an imprisonment which, unlike that it has already completed, would neither multiply the numbers of the parasite, nor yet advance its development toward maturity.

We are therefore left to comment on that close similarity of structure in the *Trichina* and *trichocephalus* which forms at present the only support for the author's inference. In no organ is this so well marked as in the intestinal canal. In both, this tube exhibits precisely the same appearance and subdivision; while in both, the segment that occupies the anterior half of the body is dilated into a series of small sacs that give it quite a moniliform appearance. It will be interesting, should future researches confirm the existing views as to the definite number and arrangement of these pharyngeal pouches; and show (what it will really be necessary to prove) that their form is neither produced nor seriously modified by any mere contraction of their sarcode lying externally to the tube beneath their chitinous envelope.

In respect to the anatomy of the *Trichina*, Luschka's researches appear to constitute one of Küchenmeister's chief sources of information. It is, however, strange that his citations of many observers do not include the really admirable descriptions and drawings of Dr. Bristowe and Mr. Rainey in the Pathological Society's Transactions (Session 1853—4); though the fact is probably due to his not having met with this publication.† In the absence of any verdict from eminent helminthologists, we will merely record our own individual conviction, that the view taken by those excellent observers as to the import of the appearances they describe, seems an untenable one: that even their own observations seem by far more easily reconciled with the decay, than with the formation, of the Trichinæ in the fatty cysts they describe. Indeed we have little doubt that, novel and interesting as it may be to find adipose tissue developed within a closed membranous cavity, the polar, as well as the intra-cellular fat they describe is a mere indirect result, or local accident, of the decay of these worms. Whether the membrane that sometimes surrounds and isolates the adipose tissue is exclusively the product of the human muscular mass, or of the parasite, it seems impossible at present to decide. But in any case it is difficult to avoid the impression that they have misinterpreted their own excellent observations;—that they have, so to speak, spelled them backwards; and have allotted the series of pheno-

* Annales des Sciences Naturelles, vol. xvii. p. 63.

† If the Reviewer (as a stranger to this Society) might take such a liberty, he would suggest a more liberal distribution of its Transactions to Foreign societies. At present they are printed, but scarcely published as their great value demands.

mena they so ably describe and illustrate to the wrong end of the life of the *Trichina spiralis.*

The *Ancyclostomum duodenale* is too important a member of the Nematoid class to be left without notice, from its extreme frequency and danger of its presence in Egypt and other tropical climates. Its length is about one-third to half an inch, its width about one-twentieth its length. Its head has a rounded apex; and its extremity, which is bevelled at the expense of its posterior surface, is provided with hooklets that occupy converging papillæ. The mouth contracts to open into a thick, muscular pharynx, which, widening as it passes downwards, ends, after occupying about one-seventh of the body, in the intestine. The sexual differences of the male and female are very interesting. Its pathological significance is chiefly due to the hæmorrhage caused by these parasites: which are often present in thousands between the valvulæ conniventes of the duodenum, jejunum, and ileum; and not infrequently in the submucous areolar tissue. In short, the physician practising in Egypt must never forget that the chlorosis of this climate is often the result of repeated and small hæmorrhages from the intestine, caused by these parasites. Turpentine, as Griesinger points out, promises to be the best remedy, both as a styptic and as a vermifuge.

The *Filaria medinensis* receives a consideration which will rather astonish those of our readers who are unacquainted with the peculiar manner in which the modern German sometimes studies the sacred writings. For out of about twenty pages devoted to this interesting parasite, about ten are set aside to its historical mention in various ancient authors. And no small share of the latter may be regarded as an attempt to prove that the "fiery flying serpent" which the Bible describes as sent among the Israelites in the course of their desert journey, was nothing more or less than the filaria! This amazing specimen of *exegesis* we shall not attempt to analyse. Whether our author's Hebrew be good or bad, it is hardly necessary to inquire; though from the remarkable care and distinctness with which it is brought forward, one is inclined to suspect that it is about on a par with the classical knowledge of Latin the display of which sometimes throws country gentlemen into ecstasies at agricultural meetings. In sober earnestness, such disquisitions are, to our mind, no legitimate object of criticism. The most placid of microscopists or chemists could scarcely bring himself to bestow a minute examination on the saliva that had just been wilfully spat into his face. And the man who attacks and insults the creed in which many (if not all) of us affect to live, and hope to die, commits a crime against all social truth and morality which goes up to a higher court than any we can imagine ourselves to preside in.* We will only hope that any one who may see fit to translate this otherwise excellent work, will (if only out of deference

* A single sentence is all that we shall adduce to justify these remarks:—"It may be supposed, either that Moses desired by the image of the (brazen) serpent to warn them against the danger of breaking off the worm, and to indicate that only he could recover who extracted, or had extracted from him, a creature like the uninjured serpent; or it is an indication, that they could only be assisted by a brazen instrument, probably a kind of cutting knife (of brass, because the Egyptians used knives of flint-stone), or a red-hot iron, such as is still made use of by the Abyssinians to open the abscess containing the filaria; and that Moses, by the brazen serpent, wished to make his countrymen more patient under the operation."

to our English feelings) remove this very silly and superfluous part of it: treat it, in fact, as a *Filaria* that has unfortunately crept into the body of the work, but which can be easily turned round one's finger, and so gently extracted.

By a not uninstructive fatality, the section on the *Filaria* is, in all other respects, one of the most meagre in the book. Our author considers that "the origin of this worm in the human body is still veiled in obscurity." We can venture to assure him that his own remark, a little further on, "it is an important circumstance that the English officers, who never went about with naked arms or feet, and never slept on the earth, remained free from the worm"—that this remark, we say, contains all (and more than all) that he seems to suspect. The observations of our zealous and able Indian colleagues* seem pretty conclusively to have established the most important facts in reference to its history and development, though they leave many details to be discovered by further researches.

The first stage in which we find the *Filaria medinensis* in the outer world is that of a minute worm. In this latter condition it has a length of about $\frac{1}{60}$th of an inch; and a breadth which, nowhere more than $\frac{1}{25}$th of its length, dwindles in the posterior fourth of its body to an invisibly fine point. Its usual haunts are the soft muddy shores or bottoms of tanks. But after the heavy rains in the regions where it is endemic, it may be found almost anywhere in the position these have left it by evaporation; a fact which obviously accounts for much that we know respecting its endemic distribution in various parts of Sennaar and Arabia, and especially explains its frequency in the marshy or dry beds of pools or wells, as well as rivers and torrents. It is therefore to the naked flesh of the person brought into contact with them that the parasites attach themselves, probably penetrating the sweat ducts, the calibre of many of which ($\frac{1}{1200}$th of an inch) would readily admit of their entry. And hence Macgregor's table of 172 cases—out of which 72 per cent. affected the feet, 20 per cent. the legs, 7 per cent. the thigh, and scarcely more than 1 per cent. the scrotum or hands—is easily explained by the circumstances of this inoculation: as is also its prevalence on the backs and shoulders of water-carriers. The period of incubation generally ranges between twelve weeks and twelve months; the longer duration probably representing the minimum of that diffuse suppuration or abscess in the areolar tissue, which first calls attention to the malady. A single bath in a tank has been often known to affect three or four persons at this distance of time, even though they have travelled hundreds of miles apart from each other shortly after bathing in company.†

This tank-worm, which is generally smaller and more slender than the young Guinea worm, has in all other respects the closest resemblance to it: is, indeed,

* Amongst these we may specially allude to a brief but pithy note "On Dracunculus in the Island of Bombay," by H. J. Carter (with a plate), in the Transactions of the Medical and Physical Society of Bombay, for the years 1853 and 1854: and to the descriptions of Forbes (Calcutta Medical and Physical Journal, vol. i. p. 215) and Duncan. (Id. vol. vii. p. 273.)

† The writer of this article lately had an instance of this kind brought under his notice by one of the sufferers, an officer of the Indian army. Here the period of incubation was twelve or thirteen weeks. In some officers, subsequently affected from the same tank, the period was seven months.

"Identical in form, colour, and general appearance. Like the young Guinea-worm, when fresh, it is very active; twisting and twirling about, seeking shelter or concealment in small pieces of silty *confervæ*, never quiet till it gets embedded in them; and frequently holding on by its tail, as if the latter were prehensile, or entangled in the mass by its temporary curvature. It swims after its head, but can fix its slender extremity to an opaque substance, and work the body into it. As the water evaporates, the tank-worm loses its energy; and perishes altogether, as it dries up or becomes putrescent: a few minutes in either state being sufficient to arrest its vitality irretrievably. Under similar circumstances the young Guinea-worm appears to be scarcely more hardy:* in short, both have extraordinarily little tenacity to life."†

The nutrition of the tank-worm and Guinea-worm can scarcely be followed into those details in which alone any contrast would be very important. But it can hardly be doubted that the parasite, bathed in the rich nutritional fluid of the human areolar tissue, must derive much of its nourishment from this fluid by endosmose through its own integuments. The size of the Guinea-worm might on such a view constitute a true hypertrophy—a direct result of the advantageous circumstances in which those Filariæ are placed which find their way into a human body. No doubt these circumstances are in some degree analogous to those which regulate the development of the Cestoid immigrant in the human intestinal canal. But the parallel may perhaps differ in one very important respect. The final habitation of the Cestoid worm, whether it raise the parasite an additional step in its general development (as in the Tænia), or convert the sexless, barren neuter, into a teeming parent (as in the Bothriocephalus)—in either case, however accidental as respects the individual, it seems to be quite essential to the species. In other words, without the intervention of that phase of life which implies this habitation, there is no reason to suppose that ova could ever be produced, or the species kept up. But it seems not impossible that the filaria may perhaps possess a very different relation to the human race. The Guinea-worm is probably (as Mr. Busk‡ has ably pointed out) the nursing or proligerous individual—the impersonated uterus—of the filaria: the analogue of the *Cercaria* in the *Distoma*: the middle of an alternate generation, or rather of a series of metamorphoses, both extremes of which remain unknown to us. Indeed, the differences between the tank-worm and the Guinea-worm scarcely amount to more than such a phase of development as this view would imply. But the tank-worm itself is found in such vast numbers§ on the soil of the regions where it is endemic, after the heavy periodical rains, that, considering the rarity of the disease, it is almost impossible to suppose all these individuals can have been even indirectly derived from the human body. The same remark will still more forcibly apply to many of the desert solitudes in which the Guinea-worm is often acquired. The fact that, in so many ages, the latter has not been propagated by infection beyond the limits of particular districts or countries, evidently points to the alternative,—either that the earlier (probably Infusorial) stage of the parasite's life is only possible under the conditions here present, or that a similar law restricts

* Mr. Busk's observations, however (Transactions of the Microscopical Society, vol. ii. p. 65), indicate a greater tenacity of life in this climate.

† H. J. Carter, *loc. cit.*

‡ Transactions of the Microscopical Society, vol. ii. p. 65.

§ H. J. Carter, *loc. cit.*

that unknown stage of existence which succeeds the extrusion of the young Guinea-worm, and raises it to the grade of development implied in the production of sexes, and finally of ova. In any case, there is at present no reason to suppose that a further infection can take place; or that a young Guinea-worm, extruded from one human being, can penetrate the tissues of another, and reproduce a nursing individual like that in which itself was once enclosed. The act of infection probably only takes place through the intervention of a whole cycle of metamorphoses, including at least one act of true generation. And if the above numerical statements be valid, attempts at prophylaxis may be almost reduced to covering (oiling?) the exposed limbs of the healthy in these regions.

The space at our disposal will not allow any notice of the careful description Küchenmeister accords to the second group of parasites, distinguished by the possession of transversely striped muscles. The various Itch-insects of man and the domestic mammalia are very well delineated; and even the lively and amusing Flea, as well as the common (alas! too common) Bug, receive all the attention they so perseveringly claim. We hoped to find something about the prophylaxis and treatment of the former of these two insects. But our author is right. No man can altogether avoid its attacks: and no amount of instruction would teach many people (otherwise extremely able and dexterous) to catch fleas. Of this useful and agreeable art we may truly say, *nascitur non fit*:—caution, dissimulation, promptness, daring, coolness, and a host of other valuable qualities, can alone enable any one to become a successful hunter of these small deer. But though few in our profession can practise among the poorer classes without making the intimate acquaintance of these curious creatures, we are aware that dignity forbids our inquiring further into their history and habits. Were it not for such a *veto*, into what queer by-ways of natural history might we not seduce our readers. If mere ganglia sufficed for authorcraft, how interesting an autobiography even a Louse might write! The days of solemn meditation, attached by one claw (the finger locked *behind* the thumb) to the base of a long and strong hair: the grave domestic life: the sebaceous or epithelial materials that serve the economical family *pour tout potage*: the large egg-sac, carefully slung round a gigantic tree in the shape of a hair, hanging defenceless, but dangerous; *gare à qui la touche!* The martyrdom the attached sinecurist will undergo rather than leave its post, clinging to any hair it may chance to grasp long after the cruel physiologist has retrenched limbs, body, and each successive joint of the particular claw! Lastly, the delicacy of its respiratory tastes: the stern dictation of the residence of some species by the thinness or thickness of the hair of particular regions; and the ease with which all are amenable to the deadly (though flattering) process of inunction! But we refrain from entering into any subject so suspiciously amusing.

The *Vegetable parasites* form a second (and, of course, smaller) section of Küchenmeister's work. Of much less interest on the whole than the Animal parasites, we can yet recommend the descriptions given by our author as careful and explicit enough to form a good text-book on the subject. Of course, there are many details in respect to which it is impossible to expect that any one person can be acquainted with all that has

been observed. Thus, on turning to the description of the *Cryptococcus cerevisiæ* (better known as the *Torula cerevisiæ*, or yeast plant), we find no mention of the fact (discovered, we believe, by Mr. Hoffmann, of Margate) that this vegetable parasite is really identical with the *Penicillum glaucum;* and, under favourable circumstances of exposure to air, developes the spores that prove this identity.*

In like manner it would not be difficult to add one or two species to the list of *Fungi* described in the work. But on the whole, the volume bears evidence of a praiseworthy industry, which has been so far successful, that it would not be easy to show any omission of importance. Indeed, whatever future additions may be made to our knowledge of these vegetable parasites, it is probable that many of them will be rather connected with phytology than with practical medicine. The distinction between a parasite which does destroy structure, and one which does not, though quite arbitrary and unscientific, will probably, in the long run, turn out to be intimately connected with those disturbances of function that chiefly attract the attention of the physician. The *Achorion*, or the *Trichophyton*, which bring about a definite lesion in the tissues of the hair and scalp, claim, *ipso facto*, an amount of attention few would be likely to devote to the fur of a feverous tongue, which may be cleaned off by the first mutton-chop that is masticated during convalescence. In like manner, just as the male† *Sarcoptes scabiei* is not only a smaller and more short-lived animal, but one which does not burrow in the human skin like the female, so this important distinction which is here connected with the sex, is, in some of the Acari that infest the lower animals, related to the species. The horse, for example, is troubled with two or three species of Acarus; some burrowing in the skin like the *Sarcoptes* of the human subject, some merely infesting the hair of the animal. The latter, when transplanted to man are comparatively harmless; while the former repeat the well-known symptoms of itch. This difference is, indeed, strictly analogous to one which may be traced in the *Pulex irritans* and the *Pulex penetrans*.

It is true that we cannot be said at present to know precisely which *Fungi* possess such a direct pathological significance. But this part of the inquiry can hardly be settled by the microscope alone. While supposing the medical evidence decides (as it seems, for instance, to have done in the case of the *Sarcina ventriculi*) that the parasite neither implies nor produces any specific derangement of the part of the body in which it is found, it will scarcely interest us whether the genus includes but one species or fifty. In fact, to speak candidly, the path of the phytologist will here diverge from that of the physician, towards a region which any one who rightly appreciates the true responsibilities of our profession, has no call to explore.

Such an allusion to the motto "ὁ βιός βραχὺς ἡ δε τέχνη μακρή," may well remind us that we have hitherto said nothing about the therapeutic part of the work we have thus briefly noticed. Its only fault is that of too

* From an observation of Dr. Hassall's, detecting this plant in the stomach, we incline to conjecture that such a development may exceptionally take place in the fermenting contents of this organ.

† Kraemer, in the Illustrirte Medicinische Zeitung. Heft 2. München. 1855.

great a copiousness, or rather too small a discrimination, of remedies. The herring cure for tape-worms, the use of a pitch plaster as a depilatory, and other venerable modes of treatment of like doubtful efficacy, distract the reader's attention from the extremely valuable methods of treatment with which they are intercalated. Many of these remedies well illustrate that physic has its fashions as well as dress; and that the undue popularity a drug often acquires on its first introduction into the Pharmacopœia, is in many instances compensated by an equally unjust oblivion when the gloss of novelty has once worn off. Perhaps there are few better examples of such a contrast than may be found in the remedies for tape-worm. When Madame Nouffers first sold her secret remedy of the male fern to Louis XV., the price she received is sufficient evidence of the value attached to it by the ministry of this unhappy monarch. By and by, finding that it was less certain against the Tænia than against the Bothriocephalus (a statement which applies to the comparative efficacy of all the ordinary anthelmintics against the two), there arose a prejudice that its efficacy was entirely confined to the worm which was the more amenable to its influence. And ever since that time, this statement, first brought forward by the great authority of Bremser, has been repeated by writers on therapeutics, with the not unnatural result of almost checking all use of the drug in a country where the Bothriocephalus is little known. It is especially interesting to notice the comparative neglect into which this most useful remedy has gradually lapsed, because scarcely a month passes by without some sensible practitioner in the country coming forward to vouch for its efficacy in one or other of the Medical Journals. And we have reason to know that there are two or three physicians to large London hospitals who have for years scarcely ever had occasion to adopt any other remedy than this time-honoured anthelmintic, recommended by Theophrastus 2150 years ago.

Review IX.

1. *Der Harnsäure-Infarkt in den Nieren neugeborener Kinder in seiner physiologischen, pathologischen, und forensischen Bedeutung.* Von Julius Hodann, Wundarzt am Hospital zu Allerheiligen in Breslau. Nebst einer kolorirten Tafel.—*Breslau*, 1855. 4to. pp. 33.

The Physiological, Pathological, and Forensic Significations of the Uric-Acid Infarctus of the Kidneys of New-born Children. By Julius Hodann, Surgeon to the Hospital of All Saints at Breslau.

2. *Recherches sur les Maladies des Enfants Nouveau-nés.* Par M. Seux, Médecin-en-chef de l'Hospice de la Charité de Marseilles. ('Des différents états qui ont été désignés sous le nom vague d'ictère des Nouveau-nés,' chap iv.)—*Paris*, 1855.

Researches on the Diseases of New-born Children. By M. Seux, Chief Physician of the Charity Hospital at Marseilles. (Chap. iv. On the Different States which have been included under the vague Title of the 'Icterus of New-born Children.')

3. *Beiträge zur Pathologischen Anatomie der Neugebornen.* Von D. F. Weber, a. o. Professor der Pathologischen Anatomie in Kiel.

Dritte Lieferung. (Art., Icterus der Neugebornen.) *Kiel*, 1854. pp. 78.

Contributions to the Pathological Anatomy of New-born Children. By D. F. Weber. (Art., Icterus of New-born Children.)

It is extemely probable that the first contractions of the uterus after the rupture of the membranes and consequent discharge of the fluid of the amnios, so protecting to the child, exert an influence over the circulation between mother and offspring, and prepare for important changes in the course, direction, and even nature of the blood within the system of the latter. The internal organs and skin, as the placental ebb and flow becomes impeded, begin to experience the first of a more or less intense and long-continued state of vascular congestion; the vessels of the cranial contents swell, those of the thorax often become so gorged that the capillaries of the pleura and of the pericardium form extravasations, and, where the scalp is relieved from the pressure, cephalhæmatoma frequently arises. With this engorgement of the internal viscera and injection of the vessels of the skin, there are reasons for believing that changes already ensue in the qualitative constitution of the blood, changes only, of course, to be perfectly worked out by the self-dependent organism assuming its new offices of respiration, digestion, and the production of caloric. The somewhat sudden alterations in the direction and nature of the blood, here alluded to, are inducted without much turmoil, so to speak, in most children; whilst in others they bring about conditions scarcely to be regarded on the one hand as *pathological*, and yet not fairly to be considered as coming within the range of the normal conditions of the new-born child. One of these results, a recoil, as it were, before the equilibrium is attained, is seen in the occurrence of the well-known *icteroid colouration* of infants, and another in the presence of *renal-infarctus* by uric acid, a far less frequently recognised condition of the new-born child. The *former*, as we have stated, has long been known, but we must add, not always well discriminated; for under the terms *jaunisse des néophytes* of Sauvages, and *icteroides corporis infantum* of Juncher, things very different in their nature and relations have been mixed up together. The *latter* is a subject of modern inquiry altogether, and a *résumé* of our present information concerning which we shall now proceed to lay before our readers, reserving a few remarks on *icterus neonatorum* for the second place.

If the kidney of a new-born child is divided from its convexity towards the pelvis, a little short of complete separation, so that the two halves may be kept together at the base when folded back, the pyramidal masses of the tubuli cut through from the cortical layer down to the papillæ, are frequently observed to be filled with a bright chrome-yellow coloured substance. As numerous tubuli are opened into by the section, the pulverulent deposit appears to be as if free, and which, either by a gentle scrape of the scalpel, or the flow of a little water, may be easily removed. The latter being effected, the remaining deposit is then seen to be retained in the unopened, engorged, infarcted tubuli. Under the microscope, the removed deposit appears composed of brownish-yellow irregularly-rounded scabrous masses, which break up under pressure into amorphous granules,

like those of the urate of ammonia. The larger masses are also more or less mixed with epithelial cells and membrane of the tubuli. In two cases, pure forms (cylindrical and rhombic) of uric-acid crystals have been seen by Hodann; and Virchow has referred to the occasional occurrence of a violet colouring matter, due to the presence of some of the constituents of blood. Hesling has further described spindle-shaped cells, having a vesicle at their termini filled—as the epithelial cells are said to be by Hodann—with the amorphous granules. The chrome-like deposit does not appear to be soluble in alcohol or in cold water, but hot water seems to partially dissolve it. This, according to Schlossberger, is proof that some of the uric acid is in combination with ammonia. The caustic alkalies also dissolve it. If a small portion be placed on a porcelain dish, and heated, and nitric acid then added, a beautiful red colour is developed, as carbonic acid and nitrogen are given off with effervescence. This latter or *murexid* test, indicates the presence of uric acid. In some cases, however, the chemical demonstration of uric acid is by no means easy, or at most only a negative result is arrived at. Both Hodann and Müller (of Breslau) have experienced this. The former advises, as the more satisfactory method, the following procedure:—*

"The kidneys being divided, and the presence of the infarctus determined, they should be dried either in the sun or by the heat of the stove. They soon shrivel up to a dark-brown skin, especially when placed on a disc of glass. The pyramids become only barely discernible as roundish elevations, but the *infarctus* is as evident as at the time of section; the striæ indicate the position of the dried-up corrugated uriniferous tubuli: the former are no longer of a yellow hue, but rise up from the dark-brown ground as blood-red yellowish stripes. Thus prepared, the object will last for years. A few strokes are now to be made, in a shaving manner with a fine knife, obliquely through the pyramids. The shaved-off particles—perhaps amounting to $\frac{1}{20}$th or $\frac{1}{10}$th of a grain—are to be allowed to fall on a porcelain capsule, then moistened with a few drops of distilled water, and are to be boiled over the flame of a small spirit-lamp. With the help of a fine knife or needle, the boiled-out parings are to be removed, a small quantity of nitric acid mixed with the residual fluid, and the effervescence having ceased, a drop of solution of ammonia being added, the purple or carmine red murexid-colour immediately appears." (p. 10.)

Such are the general characters of the *uric infarctus* of the kidneys of the new-born child. In a few instances it is of brown, or brownish-red, or straw colour; according to Hodann, a chrome-yellow is always its original hue, the darker and less frequently occurring shades being due to commencing decomposition.

"The commencing and disappearing infarctus are easily to be distinguished, particularly by means of a lens, though the naked eye will suffice. . . . .

"At the commencement, the termini of the efferent tubuli are filled, the calyces and pelvis are empty, showing no trace of colour in the fluid they contain. When the infarctus is at its height, the tubuli are filled not only so long as they run in a parallel direction, but also where their undulatory course indicates the boundary between cortical and medullary substances. In the calyces and pelvis, single pollen-like chrome-yellow granules are, as Cless correctly observes, already to be seen. As the infarctus disappears, much the same phenomena are witnessed as at its commencement, with the exception that the calyces, pelvis, ureter, and even bladder—nay, further, the anterior circumference of the prepuce in boys—

* We have prepared a kidney for permanent demonstration, and applied the murexid test satisfactorily as thus indicated.

contain the excreted matter in greater or less quantity. At the commencement, the fluid in the tubuli, behind the *infarctus*-material placed at the outlets of the papillæ, holds suspended the insoluble particles; towards the disappearance, the fluid between the papillæ and cortical matter is generally void of them. On pressure of the papillæ, the powder escapes, and fills the calyces." (p. 8.)

Both Virchow and Hodann comment on the lengthened period during which the deposit resists decomposition. The latter found that after a divided kidney had been exposed for forty-five days, and the whole had become "pappy," the infarctus injection, though much deeper in colour, was plainly visible. At a later period the granules were recognisable by the unhelped sight, even after decomposition had proceeded for three months. In the latter case the granules appeared like a fine powder upon the *débris* of decomposition, which had subsided to the bottom. The period and frequency of occurrence, and some other relations of the *infarctus*, are next proper to be considered. It is due to the following writers to state that the profession is indebted mainly to their labours for its more exact information upon these matters: viz., Cless, Engel, Schlossberger, Virchow, Hesling, Martin of Jena, Hoogeweg, and particularly Hodann of Breslau, whose researches we have just passed under notice.

The number of cases microscopically examined with the above purposes in view, and their details, so far recorded, are 427 in number; of these—

(a)	113	were dead born	. . . . .	and showed no trace of infarctus.	
(b)	1	died during birth	. . . . .	„	traces of infarctus.*
(c)	31	„ soon after birth	. . .	„	in 1, traces of infarctus.
(d)	51	„ during the first day of life		„	in 9, infarctus.
(e)	157	„ between the 2nd and 14th day		„	in 81, infarctus.
(f)	74	„ between the 14th and 60th day		„	in 27, infarctus.
	427				118

Now if we take alone those children (d, e, f,) who lived a few hours and upwards, we find that forty-two per cent. ($\frac{282}{117}$) exhibited renal infarctus, and that a broad line of demarcation must be drawn between infants who may fairly be said to have lived, and those who have not survived the birth. Two examples in the above table, however, viz.,—b, c, appear to militate against the doctrine that the presence of the infarctus would prove the independent vitality of the child; but this certainly must be said, that no case has yet been recorded in which the infarctus has been observed in the child before the act of labour has commenced, and that out of 427 children, not one born dead showed its presence. Further analysis of the histories of the cases above quoted shows that it is not until three-fourths, or eighteen hours, of the first day of extra-uterine life has been attained, that the infarctus will probably be found, and that traces of it may continue even to the end of the second month. We have not been influenced by the very general observations of Weber, Bednar, and others, upon this subject; the latter, however, it may be proper to observe, states† that, in every fourth examination of the bodies of new-born children and infants at the breast, taking place between the fourth and seventy-sixth day, he has found the deposit. We shall return to this point again. Relative to the diseases from which children pre-

* Since this article was written we have ourselves observed an analogous case.

† Die Krankheiten der Neugebornen und Säuglinge, &c. Dritter Theil, s. 189. Wien, 1852.

senting the infarctus have died, and the production of the latter, it does not appear any special connexion can be shown. Bednar is also of this opinion, at least he says he found the deposit in union with "the most different diseases" (op. cit.). General debility and atrophy, atelectasis, trismus, and convulsive maladies, apoplexy, pneumonia, icterus, &c., have been found in connexion with it. The only one of the above and other affections which at all inclines to an intimate relation, is *trismus*. The whole of Schlossberger's cases who died from this affection (five in number) had uric infarctus, as did likewise those (three in number) of Hodann. The examples of the latter, recorded by Charcelay (though reduced by him to a form of albuminous nephritis), were all in union with some convulsive disorder. A close connexion between *icterus* and the infarctus has been affirmed by some; but in none of Hodann's cases of the latter was any form of icterus present, whilst in a high degree of icterus the infarctus was not to be found. Hodann observes that:—

"Its connexion alone with one diseased condition—viz., *urolithiasis*—we cannot deny; because when the latter makes its appearance in early infancy, it is undoubtedly related to the infarctus, now become of pathologic import, and giving rise to the condition in question." (p. 18.)

The structural changes found after death coincided with the diseases above mentioned. Beside these, however, were found congestion of the contents of the cranium, even in many cases slight extravasations, and these in children the least to be suspected of such from their marked external atrophy. Both Schlossberger and Hodann agree in this, as also in the occurrence of occasional ecchymosis within the thorax. In the gall-bladder, according to Hodann, a reddish-coloured bile was generally present, in which the broken-up blood-corpuscles were demonstrable. Schlossberger found the reverse of this. Hodann observed the kidneys themselves to be—as the rule—of normal structure, always full of blood, and the cortical substance constantly darker coloured than the tubular matter. Frequently the two were separated by a narrow, dark, blood-red line. On the other hand, Schlossberger found the renal tissue generally anæmic, and but very rarely hyperæmiated, it appearing to him as if congestion of the organ excluded the infarctus. The interesting question may be now asked—is this uric infarctus of the kidney to be regarded as of physiologic or of pathologic moment? Before replying to it, it is necessary to inquire into the more exact seat of the origin and locality of the infarctus. As ordinarily seen, it is no doubt situated in the *tubuli recti* of the kidney. But is the deposit first formed there? If we adopt that view of the function of the renal organ which regards the cells lining the uriniferous tubules convoluted and straight as the instruments by which the solid matter of the urine is elaborated* (Bowman, Carpenter, &c.), and the *corpora Malpighiana* as the parts undertaking "the transudation of the superfluous fluid through the thin-walled and naked capillaries of which they are composed," we must regard the infarctus as arising in the place where it is usually observed. But this is not the theory adopted by Ludwig and Valentin, nor is it exactly that of Kölliker. In reference to this question, Hodann remarks:

* The reader is referred for some remarks upon this subject to the second volume of Dr. Morehead's Clinical Researches on Disease in India, p. 214 *et seq.*

"It thus appears that a diluted urine is formed in the Malpighian bodies; and that in the so-called convoluted canals (where greater vascularity secures to them a higher signification than that of the straight tubuli,) reciprocal actions ensue between the blood and the fluid coming from the Malpighian bodies, and that here the urine is perfected. It is probable that in the convoluted canals very important constituents of the urine first appear, *e.g.*, the greater portion of the urea; and in them also (considering the frequent degeneration of their epithelium), it seems likely that certain substances of the urine, *e.g.*, the colouring principle of it, are prepared." (p. 19.)

"As a result of investigations hitherto made, and from my own dissections (microscopic), I can but believe that the infarctus is formed in the convoluted uriniferous canals, and is first retained in the *tubuli recti*, in order to be afterward thence discharged. Whether it originates in a deposit from already-formed urine, or whether, being secreted from the vessels, it is only mixed with the diluted urine of the Malpighian bodies, is a point which shall be afterwards considered." (p. 21.)

In regard to the deposit being viewed as the result of a *physiological* or of a *pathological* process, we would observe that Schlossberger in his first essay (1842) was rather disposed to consider it as one of the latter; but in his second tract (1850) he appeared more doubtful about it, though still rather inclined to his first opinion. Virchow is decided as to its physiological character, and so are Hesling, and Martin of Jena. We shall allow Hodann to speak for himself:—

"After years' study of the matter before us, I am obliged to declare for the physiologic character of this excretion. Had it a pathologic import, it should (as before observed) be constantly found in connexion with certain diseases, and not as present with this and that affection, and then again absent from them. The reason of its being at first regarded as a pathologic product, clearly arose from the circumstance that no healthy child dying, diseased ones only could be examined. Engel declared, almost at the first, that it belonged to the normal condition, and also appeared in children who had died a violent death. It is much to be regretted that he did not express himself more fully on this point, since his so definite remark would seem to result from extensive experience. If the excretion appeared as a physiologic act at a determined time after birth, and vanished at another equally determinate, the solution of the question would be easier; the infarctus would then only occasionally be found when death happened to ensue during the period of its existence. But since we find it commencing, persisting, and disappearing; and since (though with considerable trouble) we can follow the discharge of the urates in living healthy children, we must view it in a physiologic light until its opposite relation be distinctly proved." (p. 25.)

Is it possible—it may be asked—for the infarctus to be a cadaveric product, or to be formed in the "agony," like certain cardiac clots and effusions into serous envelopes? Hodann replies—

"Supposing it to be so, it ought to be found in the bodies of all new-born children, or of those who die very young, or else we must admit that it is only in individual cases that such a physiologic or pathologic condition of the urine is present which necessitates its formation during the agony—an admission which would still more closely limit us to an inquiry as to what such condition may be. But I am decidedly of opinion that even now this question is satisfactorily answered in this—that the infarctus cannot be a cadaveric product, since it has been observed both at its onset and at its termination; and a series of investigations is before us which proves to a certainty its excretion during life." (p. 23.)

The explanation of the process (viewed physiologically) leading to the infarctus, is, according to Virchow, to be sought in that early and important change in the blood-plasma, which the sudden assumption by the child of the functions of respiration, of digestion, and the generation of caloric, immediately necessitates. So soon as the first steps in these great offices are made, the constitution of the circulating fluid becomes altered, important mechanico-chemical reciprocal changes taking place between its qualitative elements. Albumen, fibrin, &c., are destroyed, and urea, hippuric acid, and urate of ammonia remain as *débris*, the latter salt being, according to Virchow, a kind of abortion of urea, which is thrown out from the uriniferous tubes, as is also the case in adult life after great turmoil and important revolutions in the system, *e. g.*, in intermittent fever, general catarrh, rheumatic and gouty seizures. Hesling, Martin of Jena, and Piper agree to a great extent to the views of Virchow. After a description of the structure and functions of the kidney, and of the characters of the fœtal urine, Hodann details the process of labour, and observes that by this act—

"Every secerning organ awakes to a different and independent activity. Already even the blood perhaps commences to develope another chemical function in the Malpighian corpuscles and convoluted canals; it begins (forced by the unusual impulse, and debarred from returning to the maternal system) to produce the more solid constituents of the urine; and first among them—at the expense of the albumen—the uric-acid salts, as the forerunners of urea. This activity may proceed to such a height that the normal saturation is exceeded, and thus the urine can exceptionally let fall the urates so soon as it enters the *tubuli recti*, even during the act of labour, particularly when the latter is a severe one to the child. . . . . My opinion, that the commencement of the infarctus and the chief lever of its formation lie in the centric flow of blood, in the first hyperæmia of the kidneys produced by the act of labour, could only be overthrown if hereafter the infarctus should be found in children coming under investigation after removal by the Cæsarian section from the dying mother; or who have been removed from the maternal body before parturition had begun. Amongst my own dissections two such cases have occurred, where, after the sudden death of the mother, this juridical act was obliged to be performed; both children were nearly ripe, and afforded no trace of infarctus." (p. 26.)

Though according to the majority of the Continental pathologists who have investigated the subject before us, the uric-acid infarctus is to be regarded rather in a physiologic than in a pathologic aspect, several of them have stated their belief that its retention in the kidney may occur, giving rise ultimately to *calculus renalis aut vesicæ*. The relation between the two states may be thought to be further established by the fact, that while the half of all cases of calculus occurs before fourteen years of age are completed, it is only before four years that such calculi are formed exclusively of urate of ammonia; and that after ten years of age are attained, such calculi are comparatively rare. (Martin, De Lithogenesi, &c.; Prout.) Fortunately, the deposit is not ordinarily retained in the kidney, but is sooner or later mechanically washed along the urinary passages, discharged *per urethram*, and deposited on the "diapers." The occurrence of the latter, we find by reference to Graetzer,* to have been remarked some years since. "Feiler," says this

* Die Krankheiten der Fœtus, p. 110. Breslau, 1837.

writer—"speaks of children that have brought with them urinary calculi and sand into the world. The sand is then found on the napkins, which are not unfrequently, under these circumstances, coloured by blood." The true nature of the "spots on the napkins," well known to nurses as of occasional occurrence, and of the blood-red and carmine-coloured stains alluded to by several writers, is clearly resolvable into the infarctus deposit. Hodann's experiments, carefully conducted, are such as to leave no doubt, we think, upon this matter. Napkins of fine white linen were employed, and so arranged as to be a kind of filter for the more fluid portion of the urine; great care being taken in their arrangement, that no fæcal matter should fall on them. The coloured spots and stains on all these napkins were microscopically examined:—

"Blood-red and carmine-red spots I have never observed, but rather a reddish, or more frequently a brownish, or still more often a straw-yellow colouration. In most shades of it I could generally detect the urates by the microscope, after having allowed the coloured margin to moisten for some time with distilled water in a large watch-glass. In two cases, where the colouration was very deep, the residuum thus obtained (necessarily a very slight one) decidedly, though faintly, answered to the murexid test." (p. 23.)

Thirty-three cases were investigated; and in 14 the discharged infarctus was found, and most surely about the ninth day after birth. The transit of the deposit was likewise traced by the author step by step through the various urinary passages, to its final reception by the napkins:—

"Microscopic observation had satisfied me that the infarctus was forwarded from the calyces to the renal pelves, from the latter by the ureters to the bladder, and was thence ejected. . . . In one instance, of a boy fourteen days old, dying from trismus, the chrome-coloured matter lay like an encrusted ring around the prepuce, and permitted (as it was rather abundant) of microscopic and chemical demonstration." (p. 22.)

One of the most important questions connected with this infarctus of the kidney in the child, is that of its forensic value and import. Can it be accepted as a support to, or in any case in lieu of, what is usually called the "lung test" in medical jurisprudence? We have already alluded to some points which bear upon this question. It has not yet been discovered in children who have perished before labour has commenced. Not one of 113 dead-born infants exhibited the infarctus; and were it not for the two cases recorded by Martin of Jena,* and Hoogeweg, it might be said that the presence of the renal engorgement was proof of several hours' independent existence of the infant. In Martin's example, the child died after having breathed a few times; the meconium entered the air-passages, and suffocated it: whilst in that of Hoogeweg, the labour had been of more than thirty-three hours' duration, the child's heart ceasing to beat three-quarters of an hour before its completion. All that we yet know, certainly goes to prove that the infant *must have breathed*, for the infarctus to be found; but certainly not to show that the child was capable of living after birth, in such a sense of the term as would be of avail in a case of doubtful infanticide. On the other hand, also, the absence of the infarctus could not be

* And our own case, the kidney of which was lately exhibited at the Medical Society of London.

accepted as showing that the infant had not had an independent vitality. Elsässer, in a work* we cursorily noticed in our fourteenth volume (p. 377), thus writes, in connexion with the question before us:—

"In the numerous examinations of dead-born children conducted in the Catherine Hospital, which has now existed for twenty-five years, the yellow injection of the kidney has never once occurred. In children who have breathed, it has been found abundantly, even from the first day. Consequently, where in otherwise normal kidneys this injection is found, it may be laid down with almost absolute certainty that the child has lived; the converse proposition, however, cannot be maintained." (p. 76, op. cit.)

Weber, in the work described at the head of this article, expresses himself somewhat differently, but evidently speaks from no more precise knowledge of the matter than is to be gleaned from the original investigations previously mentioned by us:—

"In conclusion (says Weber), I have yet to observe, in reference to forensic medicine, that the 'sand' so much discussed as occurring in the uriniferous tubules of new-born children, cannot be regarded as a sign of established respiration. Although it is very often met with there in children who have lived a short time, yet, on the one hand, there are exceptions to this; and, on the other—what is still more important—it is found in individual cases where the children have died during birth." (Op. cit., Dritter Theil, p. 78.)

The following are Hodann's conclusions:—

"In forensic cases, its *absence* is no proof that the child has not breathed; its *presence* no surety that the child died at a definite time shortly after birth, but rendering it highly *probable* that death took place between the first and sixtieth day afterwards.

"If exceptionally found in cases where the 'lung test' went to show that respiration had not been established, it must yield in significance to the 'lung test;' if met with where the 'lung test' would indicate that respiration had ensued, it would support such test.

"If the lungs are putrid, or not to be obtained, or if a judgment must be arrived at through the kidneys, the presence of the infarctus would support the opinion that the infant had breathed, and render it at least probable that it had lived during the process of labour." (p. 31.)

We need scarcely say how much it is to be desired that the true value of the "infarctus test" be satisfactorily settled, as it is a sign of great durability. The renal engorgement has been found persisting for months after the occurrence of decomposition, and even for years after the kidneys have become dry and mummified. Observation will require to be especially directed to children known to have died before the commencement of labour, and to those not born, but removed by the Cæsarean section, on account of the sudden death of the mother. In concluding this subject, we feel called upon to thank H. Hodann for his very complete and interesting monograph. There are several matters of much value in relation to the topic we have been discussing in the work before us, but upon which we have not space to dilate. The occurrence of the renal infarctus in the lower animals, the characters of the urine at different ages, the fallacy to be guarded against in mistaking certain fibroid and pigment formations for the infarctus, and which appear to have misled Billard, nearly thirty years ago, who, no doubt, had then

* Untersuchungen über die Veränderungen im Körper der Neugeborenen durch Athmen und Lufteinblasen, &c., &c. Stuttgart, 1833.

the deposit before him; these and other questions are discussed at greater or less length by the surgeon of the Breslau Hospital.

We shall now pass to another condition apparently having its origin in the transitional steps made from *intra* to extra-*uterine* existence. The skin of the ripened fœtus and new-born infant in some respects approaches to the character of mucous membrane, in so far as it is delicate and highly injected. At birth the vessels of the tegumentary surface are so flooded with blood as to bestow a deep-red colour upon the child, a colour varying, however, very much in intensity, as well as in respect to the time it continues. Between the third and fifth day it disappears in many children, gradually and simply being supplanted by the natural hue of the skin. In others, during this change, when the red colour has been of medium depth, on pressure being made with the finger, a faint orange tint is produced, but which, on the pressure being removed, is replaced by the fading red. In some of the latter cases, towards the decline of the redness, a yellowish or orange tint appears as if suffused throughout it. In those instances where the redness is intense, as the colour passes away the whole skin of the infant frequently presents a distinct yellowish-orange or jaundice-like hue before the natural white colour of the integument is attained. This "icteroid colouration," however, is rarely of a true yellow-saffron hue, but rather of a "raw sienna" tone, with a slight dash of green in it; or in some cases, where deep in intensity, it is of a dirty orange tint. According to most observers the conjunctiva is free from colouration, the urine does not contain any of the elements of the bile; none of these are to be found in the serum of the blood, nor in any other fluid of the economy. Such examples we shall consider—following M. Seux—as forming a class (I.) distinct from the next series (Class 2), but between which we may at once confess *we* are not *always* able to discriminate. Class 2 is composed of cases which the following extract from M. Seux will fairly illustrate:—

"At a period varying from one to ten days, but more especially during the first four after birth, the skin of the infant exhibited a yellow tinge, varying in depth in different individuals. Generally of a light yellow the first day, it gradually increased in depth until it sometimes had a saffron-like appearance; the conjunctiva evinced similar shades, and the mucous membrane of the mouth was also slightly coloured yellow. The child sucked as usual, the digestive organs were not in the least disturbed, and the motions remained yellow and of the natural consistency. The pulse was normal, the skin of the proper temperature, the urine stained the napkins yellow, the face did not express pain; in fine, except for the yellow colour of the skin and of the conjunctivæ, it could not have been said that the infant was unwell. Sometimes a few days after the appearance of the jaundice, ophthalmia, with free secretion of pus, came on; the colour of the latter was like that of the skin; indeed, it was so similar that the nurses designated the affection as 'jaundice fallen upon the eyes.'"

In these cases, forming the second class, the icteroid colouration generally soon disappears, or demands but the very simplest therapeia.

Another and fewer number of examples we must group together as forming a third division. In these at any time between the second and eighth day after birth, particularly after exposure to cold, a yellow colour of the skin, and generally of the conjunctivæ, begins to appear. Soon more or less constitutional disturbance ensues, there is constipation, and

dislike to take nourishment, the pulse varies (96 to 102, Seux), some emaciation occurs, the motions, such as they are, are pale, the urine is frequently coloured, and the skin scarcely maintains its proper temperature. In these cases, however, it will be found the rule, that under the employment of a proper therapeia a return to health will ensue in a week's time. In a few instances, it must be owned, such will not happen to be the case, for as the jaundice progresses the region of the liver becomes tumefied and painful to the touch, matters go on from bad to worse, and the child dies during the second or third week of its existence. After death, to quote Case III. of M. Seux:—

"The liver measures transversely fifteen centimètres, and ten from before to behind; it is of a reddish-grey colour. The gall-bladder is distended by very thick green bile; its internal surface presents no redness, on the contrary, it is stained green. When cut into, the liver emits a great quantity of black blood with which it was gorged. The substance of the organ breaks up everywhere with the greatest facility, and if scraped by the scalpel, becomes reduced to a pulp: it is a softened liver." (p. 276.)

A fourth, and a most important class of cases may be thus cursorily characterized: at any time within the first week of life, but mostly within the first half of it, symptoms of *mal-aise* appear in the child, with marked disturbance (varying in character) of the digestive organs. Soon a jaundiced hue pervades the skin, and is accompanied from the beginning by diarrhœa, abdominal tension, or even signs of acute and intense enteritis; or the jaundice is associated with hæmorrhage from the umbilical cord or its seat, or with phlebitis of the umbilical vessels, or with pyæmia, and which carries off the little patient. In other cases, again, what has been termed, rather absurdly, "malignant hepatitis," co-exists with the icterus, or the latter complicates *sclerema*,* or is in union with severe *muguet*, erysipelas, erythriasis, pleurisy, or *atelectasis*. The child, as may be readily believed, too frequently dies, rarely, indeed, surviving the third week. After death the anatomic changes then found vary of course with the complications above alluded to. In 45 cases recorded by M. Hervieux, the *icterus* was in union with sclerema in 31 instances; with enteritis in 15; with muguet in 5; and with pneumonia in 2. In M. Seux's worst forms:—

"The jaundice has always been coincident with enteritic inflammation, and only in two cases of twenty-six did the icteroid colouration *precede* the intestinal disorder. . . . Five children died on the fourth day after the appearance of the jaundice; one on the sixth, carried off by 'muguet,' combined with intense enteritis; the other three died, one on the fourth, one on the fifth, and one on the eighteenth day; the first two from simple enteritis, and the third under the influence of a gangrenous affection." (p. 268.)

In some of these cases the fat, bones, ligaments, cartilages, &c., (Billard, Hervieux), occasionally the sclerotic, the serous and mucous membranes, with the fluids they contain, have been found coloured yellow.

In a fifth and last class of instances may be placed those of congenital jaundice, in which either the mother being jaundiced has brought forth a jaundiced child,† or where the jaundice is produced by certain malfor-

* See on this and correlated points a valuable paper by Bierbaum, lately published in the 27th vol. of the Journal für Kinderkrankheiten.

† We were informed by the midwife at the Marylebone Infirmary, that such had occurred at that Institution (a short time before a visit we lately made to the wards, through the favour of Mr. Mushett); and that the "poor mother was quite alarmed at her yellow child."

mations, such as non-closure of the *ductus venosus*, absence of the hepatic or other ducts, as also those forms of fœtal jaundice described by Lobstein under the head of *kirrhonosis*. For examples of the latter varieties we may refer our readers to the work of Graetzer, which we have previously quoted. Now to all these varieties of jaundice, the general titles of *icterus* or *cholæmia neonatorum* have been applied most indiscriminately, and futile attempts have constantly been made to discover some single pathologic element as their more frequent or general cause. Hence we have had given as such cause, obstruction by viscidities, &c., of the gall-ducts (Baumes, Underwood, Frank, Dunglisson, Bamberger); narrowing of the biliary ducts, with suppurative inflammation of the liver (Heinke, Richard, de Nancy); accumulation in the gall-bladder of bile too viscid to pass away (Porchet); acute idiopathic hepatitis (Bouchut); passive congestion, or hyperæmia of the liver (Billard); the spread of a reflected irritation of peritonitic or enteritic inflammation to the liver (Valleix, Seux); sudden ligature of the cord (Morgagni); pressure of the liver and head during labour* (Chambers); disordered function of the hepatic organ in its transition from an office of hæmatosis to one of biliary secretion (Hervieux); to a vicarious function of the liver for the lungs, and to physiologic changes of the blood itself, &c. &c. (Virchow, Martin of Jena, and others).

From what has been already stated, it is clear that the jaundice of the new-born child may be produced by, or intimately associated with, many very different morbid conditions, but concerning which, as we are not discussing the subject in detail, it would be here out of place further to dilate upon. It is sufficient for our purpose to draw attention to the fact that forms of icteroid colouration coming under Class I., must certainly be distinguished from those of Classes 3, 4, 5, as essentially distinct in nature, and as not being cases of true jaundice at all. With respect to Class 2, we are in this difficulty,—viz., we doubt as to the propriety of maintaining it, believing that its extreme cases run respectively into I. and 3, or, at any rate, we are not always able to say whether a particular instance should be referred rather to one of these classes than to Class 2. Much of the difficulty of distinguishing between certain forms of the two types, arises from the fact that, according to our own observation,† the conjunctiva is quite as frequently as not tinged in examples of Class I., a circumstance denied by many others, and the occurrence of which is hence deemed by them as at once diagnosticating Class 2. We should, therefore, admit that we are as yet unprovided with a sure means of differentiating between *all* instances of true *icterus* and the *physiological* icteroid colouration of the new-born child. The propriety of the general distinction being made, however, has long been admitted, being first

* In a child we lately saw at the Marylebone Infirmary, the labour had been tedious, the pressure great, and cephalhæmatoma had arisen; nevertheless, the cutaneous injection was comparatively very slight, and was said to be quickly disappearing: here no icteroid colouration was manifesting itself. From further inquiries which we have made, and from what has come under our observation, we have been unable to trace a relation between pressure during labour and the intensity of the yellow colour of the integuments.

† The writer of this article would here beg to acknowledge the debt he is under to Dr. Robert Lee, for procuring him the *entrée* of the British Lying-in Hospital, in order to observe children from the birth; an advantage not commanded, of course, at the Royal Infirmary for Children.

recognised (so far as we are aware) by Seutin in 1797.* He remarked that—

"new-born children often have an apparent jaundice only, perhaps, as a consequence of a tedious labour, since he had very frequently observed children with such an appearance, but wanting all other signs of jaundice except the yellow colour." (Graetzer, op. cit. p. 120.)

What, then, it may be asked, is the nature of this icteroid colouration of new-born children? If it is not jaundice—if it does not depend upon the presence of some of the elements of bile in the blood—what is its interpretation? Is it, like the renal uric-infarctus, rather of a physiologic than of a pathologic import? According to some pathologists, a state of *polycholia* is present, an excess of bile is formed too great for the demands of the system, and which the skin is called upon to assist in disposing of for a few days. The origin of this polycholia is variously explained; it has been assumed that during the act of labour, and from the subsequent changes in the direction, &c., of the circulation, the liver becomes overflowed, as it were, by blood, and its functional activity called rapidly into action, yet with an embargo upon it all the time. Again, the liver has been viewed as an organ that can act vicariously for the lungs, kidneys, and skin, and hence that it is called upon to purify the blood and eliminate the carbon, when the duty of the pulmonary organs, &c., is imperfectly effected. A combination of these causes has been adopted by some. According to Hennig:†—

"Shortly after birth the liver is exposed to the danger of being overfilled with blood, and has besides occasionally to assume the deranged functions of the skin, and even of the kidneys. . . . By this polycholia, prematurely-born children are particularly liable to be attacked; it appears about the third or fourth day after birth, continues from four to fourteen days—longest in unripe new-born infants—and gives rise to no mischief beyond the arrestation of the oxygenised supplies to the blood accompanying the disturbed functions of the skin."

The same writer believes that besides this polycholia there is also some change in the colouring matter of the blood; but it does not appear to us that he can, more readily than ourselves, always discriminate between such cases as—following the views of some—we have placed in Class 2, and those of Class I.

In opposition to such theories as the above, it has been maintained that in the cases under consideration there do not exist signs either of hyperæmia of the liver or of deficient respiration; and secondly, it has been asserted (Lehmann) that there are no pathologico-anatomic facts which favour the view that the liver can act vicariously for the lungs; and that the separation of the carbon by the liver, as compared with that by the lungs, is so trifling, that the hepatic organ can hardly be regarded as essentially a blood-purifying apparatus, in so far as the elimination of carbon is concerned. In reference to the question of a compensatory power of some kind possessed by the liver in respect of the lungs, we may, *en passant*, refer the reader to a note in Dr Stokes' work 'On Diseases of the Heart,' &c. p. 259, and to some observations at p. 9 of the Introduction to 'Dr. Morehead's Clinical Researches on Disease in

* Beiträge zur ausübenden Arzneiwissenschaft.

† Lehrbuch der Krankheiten des Kindes, &c., p. 112 *et seq.* Leipzig, 1855.

India." In the opinion of some recent pathologists, the more satisfactory explanation of the icteroid hue of new-born children is that which, leaving the liver and bile entirely out of the question, regards it in the same light as the renal infarctus, *i.e.*, from a purely physiologic and *blood* point of view. But even here there is much obscurity. Levret, Grisolle, and others, consider the colouration more as a kind of ecchymosis, in the progressive changes of the hæmatin of the blood of which, it has its origin. To quote the words of M. Seux :—

"There truly exists a jaundice limited to the skin and related to the changes which the latter is subjected to after birth; the blood which at the moment of birth floods the integument, giving it a decided red colour, gradually retires; in doing so it assumes different colours, varying from a light yellow to a greenish yellow; these tints are evidently the result of a portion of the materials of the blood slowly resorbed, just as we observe in ecchymosis." (p. 250.)

So Bamberger,* speaking of *icterus neonatorum*, observes, that in certain cases—

"It arises from the progressive colour-changes of the hæmatin which, in consequence of the extreme cutaneous hyperæmia accompanying the first moments of life, is deposited in the skin, and remains there. It is analogous to the pigment-stains which continue after sinapisms and other cutaneous irritants."

In further elucidation of the cutaneous injection, M. Prosper Despine remarks (Seux), that it is mainly seen, or at least is chiefly followed by, the yellow colouration when the umbilical cord has been suddenly tied without the precaution of allowing it to bleed to a slight extent, which relieves the congestion of the skin. The following observations of Weber appear to us as worthy of extract—

"That it is connected with disturbance of the circulation, *at least in the skin*, produced probably by the abnormal prodromata of the act of labour, is further apparent to me from the fact that children having very red integuments, exhibit spots *still more intensely red* than the rest of the surface of the body. Such, for instance, are observed at those places on the brow which correspond to the frontal suture, and where, from the forcible undersliding of one frontal bone beneath the other, a fold of the skin arises during the passage through the lower pelvis. In this fold the circulation must be more or less hindered until the birth of the child. When we now observe, after a few days, that not only is the whole surface of the body (which was before very red) coloured yellow, but that these spots are especially so in which the circulation felt the pressure—when we further observe at these spots small extravasations of blood—and, finally, when we remark that this variety of *icterus* belongs to those transient forms not in the least obnoxious to life, I believe that we are justified in considering that such icterus has not its origin in pathologic changes of the liver, but that it depends on a direct pigment-metamorphosis, in part of true though small extravasations of blood in the skin, particularly at the before-mentioned spots, and in part of the colouring matter of such blood as has stagnated for some time in the hyperæmiated capillaries of the skin. The misfortune is, demonstrative evidence of this cannot be obtained; such cases never come to a post-mortem examination, so that an hyperæmia of the liver cannot be absolutely denied." (p. 44.)

Virchow takes a very different view from the above. We have already seen that he leans to the early occurrence of important qualitative changes

* Handbuch der Speciellen Pathologie und Therapie, &c. Redigirt Von Rud. Virchow. Band 6. Abtheil. i. Seite 590.

in the blood, mainly evincing themselves as a general breaking up, as it were, of this fluid. As one consequence of these changes in the blood at birth, we have (according to Virchow) the deposit of uric-acid salts (whose excretion is as rich as it is sudden) coming on during the first or second day, accompanied by considerable congestion of the kidney (Virchow, Hodann), and even occasionally by extravasation of blood, or the exudation of serum within the tissue of the renal organ. It is assumed, however, that it is the metamorphoses of the *blood-plasma* which are the bottom of the uric infarctus; but the *blood-corpuscules* likewise undergo change(!) They do so; and the visible expression of such change is seen in the *cutaneous icteroid colouration* of the infant. Martin of Jena agrees with the opinions of Virchow. We may, in conclusion, observe that, more than ten years back, M. Decaisne alluded* to an idiopathic icterus occurring in the adult, in which the discolouration of the skin was produced by some change in the constitution of the blood, and was altogether unconnected with a redundancy of bile. M. Decaisne was replied to, however,† by the assertion that such discolouration was merely *sallowness*, the result of a cachectic condition of the body in general, in which less blood, or blood of an altered character (other than connected with jaundice), circulates in the cutaneous vessels. It may not be altogether out of place for us here to remark also, that Messrs. Calvert and Moffat have lately called attention‡ to the icteroid colouration of the skin produced by the internal employment of the carbo-azotates of iron and ammonia. The patients (they inform us) became yellow as if they had a severe attack of jaundice, and not only the skin but also the conjunctivæ became coloured. The time necessary for this colouration to ensue seemed to vary, according to the individual, from two to sixteen days; the mean being seven days. The quantity of carbo-azotate generally producing it has been about a scruple, and it disappears in two or three days after the agent has ceased to be administered. Braconnot, who employed the carbo-azotate of potash, did not obtain the result in question. The writers above referred to think this was probably due to the insolubility of the salt employed, or that he (Braconnot) did not use true carbo-azotate of potash.

Review X.

Mémoires de la Société de Chirurgie de Paris. Tome Troisième. Fascicule 4. Tome Quatrième. Fascicules 1–4. 4to.—*Paris,* 1853–5.
Memoirs of the Society of Surgery of Paris.

After a longer interval than desirable, we resume our analysis of the Transactions of the Paris Surgical Society, commencing with the portion of the volume we left unfinished in our last notice.§

I. *On the Reduction of Dislocations of the Shoulder when Complicated with Fracture.* By M. Richet.

This paper is founded upon a highly interesting case which occurred

* Gazette Médicale, Mai, 1845. † Ranking's Abstract, vol. ii. p. 224.
‡ Pharmaceutical Journal, September, 1856.
§ Vide Brit. and For. Med.-Chir. Rev., vol. xiii. p. 465.

to the author, in the person of a man sixty-eight years of age, in whom the sub-coracoidean luxation of the left humerus was complicated by a fracture of the anatomical neck of this bone. He came to the hospital on the 8th of September; but replacement of the bone was not attempted until the 11th. The patient being then placed completely under the influence of chloroform, M. Richet surrounded the shoulder firmly with both his hands, so that the two thumbs rested on the acromion, and the four fingers of each hand were carried up to the summit of the axilla. By thus operating upon the head so as to move it from within outwards, the reduction was almost insensibly effected with the exertion of very little force. The fracture was then carefully set, and the bandages were retained until the end of October. The patient was seen accidentally nine months after, and was found able to execute all the movements of the limb without difficulty, while there was no perceptible shortening.

M. Richet points out how unsatisfactory the treatment of this description of accident has hitherto been; and believes that no improvement could have taken place until the discovery of anæsthetics, which, by annihilating muscular resistance, allow of the above manipulation being made with success. In luxation of the shoulder, this is indeed the only resistance to be encountered; for M. Richet believes that that attributed to the fibrous structures of the joint, or the insufficiency of the aperture in the capsule, has been much exaggerated. Numerous opportunities of examining such cases after death have always shown the opening in the capsule to be large, irregular, and jagged; while when dislocation is produced experimentally in the subject, very moderate direct pressure made by the hand on the axilla, suffices to effect reduction. Even in the case of dislocation of the femur, when, from the shape of the parts concerned, entanglement of the head in the edges of the capsule may occur, this is best remedied, not by extension, but by rotation and direct pressure. This last alone sufficed in some of the author's experiments, in which the head of the femur was dislocated, and its shaft sawn through below the trochanters—so as to simulate dislocation complicated with fracture. The general conclusion at which M. Richet arrives is, that while extension must continue as the general method of treating luxations unaccompanied with fracture, even here direct pressure (or as he terms it, *refoulement*, or pushing back) will always prove a powerful auxiliary; and is, in the case of dislocations complicated with fracture, the only procedure practicable, to the exclusion of extension.

M. Gosselin, reporting to the Society upon this paper, observes that although somewhat analogous procedures have been employed occasionally, and that the use of such direct pressure has been formally recommended by Chassaignac and Morel-Lavaillée; yet that the merits of converting what were little other than suggestions into clinical facts, and supporting them by experimental researches, is M. Richet's. He does not think that the employment of anæsthetics is so essential a feature as it is deemed by M. Richet; but that *refoulement* should be tried even when these are contra-indicated,—as, for example, during the state of nervous shock immediately after the accident. The procedure, the reporter observes, will not be applicable to all cases; for the diagnosis is often,

mally described by Nélaton in 1847, and then under the title of partial dislocation of the astragalus. This luxation may take place laterally or *backwards*, the lower surface of the astragalus lying, in this last case, upon the dorsal surface of the second row of the tarsus. Of this there is, however, only one example on record, occurring in the person of Mr. Carmichael, and related by Macdonnel in the fourteenth volume of the Dublin Journal (1835).

Lateral displacements are much less rare, although they have been described by most narrators without a distinct appreciation of their nature. Leaving out cases the accounts of which are too imperfect, M. Broca has collected 19 cases, several of these being verified by autopsies. Of these, the displacement occurred thirteen times externally and six times internally. In 1 case the nature of the cause was not stated, in 6 it was due to the direct action of a heavy body upon the astragalus, and in all the other cases it arose from a fall on the foot, that is, from an indirect cause. M. Broca finds the explanation of the operation of this latter in the fact of the sub-astragular articulation being the principal seat of the motions of adduction and abduction of the foot. In dislocation inwards there has been forced adduction, and in dislocation outwards forced abduction: and although in the cases published there is scarcely any account given of the position of the foot at the time of the accident, yet in 10 of these cases the details of the symptoms after its occurrence are sufficiently precise to enable us to ascertain that in 4 luxations inwards the axis of the foot was in a state of permanent adduction, and in 6 luxations outwards it was in a state of abduction.

Lesions of surrounding parts.—The sub-astragalian ligament is the first torn, its external fibres, however, sometimes resisting, which may explain the irreducibility of the luxation in certain cases. The two sub-astragalian synovial capsules are also partially or entirely torn, and the astragalo-scaphoidean ligament is always found ruptured to a great extent, allowing the passage of the head of the astragalus. One or both of the fibrous bundles, forming parts of the lateral ligaments uniting the astragalus to the calcaneum, are also ruptured. The rupture of the integument by the head of the astragalus is one of the most frequent complications; but although all the cases collected by M. Broca have presented it with the exception of four, it must not be considered an almost necessary occurrence. The existence of sub-astragalian luxation has usually hitherto been confounded with luxation of the astragalus, properly so-called; and when the integument has not been broken, the relative position of the astragalus has been often too imperfectly indicated to allow of more than a strong suspicion being entertained that some of these cases were examples of simple sub-astragalian dislocation. The numerous tendons in the region of the head of the astragalus often undergo rupture or displacement, and the tibial arteries may become ruptured or stretched. Fractures may also complicate these luxations. The articular surfaces of the calcaneum or astragalus may become fractured or detached, but these are of no great importance. In one case the tibia was fractured as well as the fibula, while in 6 the fibula alone was broken, all these (6 in 13) occurring in cases of external luxation.

Symptoms.—In luxation *outwards* there is usually more or less abduc-

tion; the external edge of the foot is almost always raised, the inner resting on the ground; when a wound exists, it allows of the issue of the head of the astragalus below and in front of the *malleolus internus;* the tendon of the *tibialis posticus* is either displaced or torn; and the posterior tibial artery is either ruptured or stretched. These signs are present when the luxation exists in its first degree, the astragalus still lying in part upon the calcaneum: but, in the second degree, when the two bones are quite separated, and the calcaneum becomes placed on the side of the fibula, there is shortening of the limb, and the tibia, fibula, and astragalus, together, make a projection of two or three inches externally. In the luxation *inwards*, the positions of the foot and of the wound are the opposites of those stated above, the astragalus usually lies on the dorsal surface of the cuboido-scaphoidean articulation, and it is the anterior tibial artery that may become torn or stretched.

There are two essential signs common to both luxations:—1. Whenever the head of the astragalus retains its normal relations with the bones of the leg (which may be easily ascertained, as it always strongly distends or passes through the skin), we may be certain we have to do with a sub-astragalian luxation. 2. The movements of flexion and extension of the instep, which are lost in tibio-tarsal luxation, and in total luxations of the astragalus, are preserved in sub-astragalian dislocations.

Diagnosis.—From what has been said of the signs of this accident, it is evident that its diagnosis is easy, and yet the greater number of cases have been confounded with luxation of the astragalus, properly so-called. The means of distinguishing the two are stated above. It might also be mistaken for lateral luxations of the foot, inasmuch as the tibio-tarsal luxation outwards gives to the foot a form and position somewhat resembling internal sub-astragalian dislocation, and internal luxation of the foot resembles external sub-astragalian. Here, again, if we find the head of the astragalus projecting, the luxation is sub-astragalian; if not, it is a luxation of the foot. Again, certain fractures, accompanied by displacement, may strongly simulate these luxations. Such are what M. Broca terms *sub-trochlean fractures of the astragalus*, in which, the bone becoming divided horizontally into two segments, the lower of these retaining its attachments to the calcaneum, may become displaced with it laterally, if the force has ruptured the ligaments uniting the calcaneum with the bones of the leg. The diagnosis is again to be derived from an examination of the head of the astragalus. If this is found projecting on the back or the internal edge of the foot, we are certain that it is a case of sub-astragalian luxation, and not a case of sub-trochlean fracture.

Prognosis.—This is much influenced by whether the luxation be complicated by a wound or not. In the latter case, the displacement is usually less considerable and reduction more easy. Among the 5 cases collected, the luxation was only irreducible in one: but of 17 cases complicated with wound, reduction only took place in 5 of them, and in 2 of these the patients died. Of the 12 irreducible luxations, immediate amputation was performed in 3 cases; in 1 the astragalus became necrosed, and was detached; and in 8 cases this bone was extracted, 6 patients recovering and 2 dying.

Treatment.—The rules for this are thus summed up by M. Broca:—

"1. In luxations unaccompanied by a wound we must attempt reduction, and especially by means of the knee. If our attempt fail, we must wait. In the event of an abscess afterwards occurring we must open it, and subsequently extract the astragalus—an operation which is *then* attended with remarkably little danger. 2. In luxations, when there is a wound, we must also attempt reduction; having recourse to *débridement* and tenotomy, if necessary. When reduction is not possible, the astragalus should be at once removed, as by this operation three-fourths of the patients are saved, and it is of less gravity than amputation of the leg, and has the advantage of preserving the functions of the limb. (tome iii. p. 616.)

2. Medio-tarsan Luxations.—This is the name proposed by M. Broca for displacements which may occur between the first and second rows of the tarsus, the luxation being termed total when the scaphoid and cuboid are simultaneously displaced, and partial should either bone be displaced separately. This division of the Memoir need not occupy us long, as the author states his belief that these dislocations, which the anatomical conditions of the parts render highly improbable, have, in point of fact, never yet occurred, the conditions which have been described as such having been faulty interpretations. First, as to *total medio-tarsan luxations*, passing by two cases so vaguely indicated by Petit as to amount to little more than assertions, M. Broca knows only of two others which have been published as examples of medio-tarsan luxation. Both are found in Sir A. Cooper's work, under the appellation of Dislocation of the Os Calcis and Astragalus. The first of these was only observed by some students, and so vaguely related to Sir A. Cooper as to be quite worthless. The other, observed by South under Cline, Cooper, M. Broca is convinced, inserted in his work after merely reading the appellation given it by Mr. South—viz., a luxation of the astragalus and os calcis; by which he signified a luxation between the two bones—the case being, in fact, a fine example of sub-astragular luxation inwards. Cooper, however, in his nomenclature, applied this term to medio-tarsan dislocation, and accordingly the case was so regarded by him, which it could never have been had he perused the details. But this same case (that of Gilmore) had also been observed under Cline, by Green, and was likewise communicated by him to Cooper, as an example of dislocation of the astragalus outwards, so that Sir Astley has twice inserted the same case—once as an example of luxation of the astragalus outwards, and again as a medio-tarsan luxation!*

M. Broca passes under critical review all the observations purporting to be examples of separate luxation of the *scaphoid*, and comes to the conclusion that its occurrence remains entirely unproved, all the cases being capable of explanation in admitting a sub-astragalian luxation, and some being explicable in no other way. In denying that these luxations have ever been met with, he does not assert the impossibility of their occurrence, however strongly such assertion may seem to be supported by anatomical theory. All he now maintains is, that the history of these pretended luxations has been founded upon erroneous bases, that not a single fact demonstrates their existence, that all documents hitherto published witness against them, and that if one or other of these lesions should come hereafter to be observed, it will doubtless present different characters than those hitherto attributed to it.

* See Cooper on Dislocations, Fifth Edit., pp. 342 and 349.

Besides those we have now noticed, this livraison, completing the third volume, contains the following Memoirs, of which we content ourselves with merely transcribing the titles: Follin, On the Ophthalmoscope; Rigal, On a New Mode of Ligaturing Erectile Tumours; Giraldès, On Mucous Cysts of the Maxillary Sinus; Jarjavy, On Dilatation of the Parotid and other Ducts; and Sédillot, On Cheiloplasty in an old Case of Cancroid. Turning to the fourth volume, and passing over an interesting biographical sketch of Augustus Bérard, and the Compte Rendu of the highly valuable labours of the Society of Surgery, during the ten years 1843–53, we come to a paper entitled

IV. *Researches on the Cysts of the Wolffian Bodies.* By M. Verneuil.

M. Verneuil thus expresses himself in regard to the origin of cysts in general:—

"My researches upon cysts in general have led me long since to the adoption of a division somewhat resembling that of M. Cruveilhier and the authors of the *Compendium de Chirurgie.* We recognise cystiform productions which originate in a circumscribed accumulation of liquid in accidental cavities formed at the expense of the cellular tissue of any part of the body, by a mechanism analogous to that which gives rise to subcutaneous serous bursæ: but while we thus provisionally admit this variety, known as *serous cysts,* and which seems to us to be artificially arranged with cysts properly so called, we strongly protest against the strange abuse that is made of this word. In our day, in fact, we find it applied to any tumour without distinction, provided only that it be more or less spherical, and that without scarcely any account being taken of the nature of its walls or of its contents.

"We attribute to cysts properly so called but one single origin, the *glandular element:* and we thus form a very natural pathological family, embracing a great number of diseases, the superficial differences of which disappear before examination. Hence, we do not limit ourselves to ranging here the dermoid cysts of authors, but guided by a much wider generalization, we lay down the following propositions:—1. All organs which contain compound glands, or even simple follicles, and, *à fortiori,* the glands themselves, are susceptible of forming cysts. 2. We never meet with true cysts (hydatids, being accidental products, are here excluded) in any organs or regions in which glands are not present. 3. If a region contains several species of glands, it may also present various species of cysts. 4. The richer an apparatus is in glands, the more frequently will cysts be observed. Certain anatomical conditions have, however, to be taken into account, which it is not our object here to exhibit." (tome iv. p. 79.)

The following are M. Verneuil's conclusions respecting the special cysts it is his object to illustrate:—

"1. The Wolffian bodies, whose functions relate to the early phases of intra-uterine life, become atrophied in the human species towards the fourth month of gestation, but leave *débris* during the whole of life, which are known in the female as Rosenmüller's bodies, and are situated within the substance of the broad ligament. In man, they form a mass of canals towards the head of the epididymis, constituting the *vas aberrans* and hydatid of Morgagni. The researches of MM. Kobelt, Follin, and Gosselin, have established these facts. 2. These glandular *culs-de-sac* are liable to distension by fluid accumulating in their interior, and then give rise to cysts known as cysts of the broad ligament, and small cysts of the testis. 3. These productions have been seen by a good number of observers,—as Velpeau, Delpech, Bright, Huguier, Gosselin, Follin, &c.: but, with the exception of the last, these observers have misunderstood their origin, and have explained their formation by hypotheses that should be abandoned. 4. In the female, they acquire a variable size, from that of a pea to that of an orange, and they may be

sessile or interstitial, pediculated or free. Inflammation of the peritoneum and of the genital passages asserts an influence on their production. 5. In man they ordinarily remain very small. M. Gosselin has given a complete description of them. 6. The symptoms are absent or obscure, and in the present state of our knowledge no precise diagnosis can be made. 7. The prognosis is of little gravity, and treatment cannot be put into force. 8. Their origin is explained by the general laws which preside over the formation of cysts, and is moreover proved by direct observation. Their identity in the two sexes is beyond all doubt." (p. 84.)

V. *On the Seton.* By M. Bouvier.

In this paper, M. Bouvier advocates the use of small, or what he terms *filiform*, setons. A subsequent memoir of his upon the same subject has given rise to a very prolonged discussion in the Académie de Médecine, during which the doctrines of revulsion and irritation as held by ancients and moderns, were propounded with most wearisome prolixity.* The present paper is of a purely practical kind, the author urging the more frequent recourse to the use of the small seton as a means of counter-irritation. A peculiarity of its action is the small amount of irritation it determines in the skin, and the suppurative inflammation it induces in the subcutaneous cellular tissue. Its immediate, primary effect is of little intensity as compared with the cautery or the moxa, while its consecutive effects are not less considerable. In no other mode can suppuration be so easily maintained for an almost unlimited period. Except in cases in which it is desired to excite immediate energetic action of the skin, the seton procures a not less effective derivation than that which is obtained from other excretories which induce a destruction of the skin, while it is superior to them in its prolonged and continuous action.

M. Bouvier believes a principal reason why so valuable a means is so much neglected, to be the formidable mode in which it has hitherto been employed, and that much smaller setons and needles may be used than it has been customary to employ. Thus, an ordinary suture-needle, either straight or curved, providing its extremity be flattened and sharp at the sides, very well suffices. The size should vary, according to whether one or more threads have to be carried, always remaining much less than the ordinary seton-needle, and a spring at the larger end should receive the thread in place of an eye. The seton may in a few days, if desired, be enlarged by the addition of other threads. The passage of such a needle causes but little pain. M. Bouvier has made many trials of the various substances used as setons. Those which are most permeable to the pus favour best its discharge, such as slips of linen, or threads in juxtaposition. Of such substances, silk is least irritating, cotton somewhat more so, and wool most of all. Catgut also produces an abundant suppuration. All things being equal, the irritation is proportionate to the amount of distension of the tissues induced by the seton. Impermeable setons, which need not be removed in order to clean them, are of such simple and easy employment, that they should always be preferred, except in special cases indicating the use of the others. Among these M. Bouvier mentions with approbation threads of vulcanized caoutchouc, ordinary thread enveloped in caoutchouc, and gummed silk. Metallic substances, owing to their

* See Bulletin de l'Académie de Méd., tome xxi.

polish, excite but little irritation, and the author has employed small gold, silver, or gilt chains, fastened at their extremities, as precautionary setons, to be excited into activity if required.

In regard to the *direction* given to a seton, this should always be such as to bring one of the orifices in the skin into a state of declivity. In the usual mode of applying a seton to the neck this rule is violated, and the seton being placed horizontally, the pus tends by its weight to fuse below the level of the two apertures, and to form collections there that are emptied with difficulty. Were the seton made longitudinally, as it used to be by the old practitioners, this inconvenience would be avoided. By throwing the head back and employing curved needles, setons are as easily passed longitudinally as horizontally; and both the author and M. Mélier have adopted this mode of passing them with advantage. In cases requiring it, two longitudinal setons may be passed side by side, or multiple filiform setons may pass in different directions, the cruciform being a favourite with M. Bouvier.

VI. *On Stricture of the Urethra.* By M. Alphonse Guérin.

This is a memoir of some importance, inasmuch as it is based upon the examination of the urethra of 100 subjects brought to Clamart, having signs of gonorrhœa upon them when they died. In one-half of these cases stricture was met with, and it is to the mode of formation of this, and the rules of practice flowing from its consideration, that the author directs our attention. He was much surprised to find the mucous membrane quite devoid of all trace of cicatrix and false membrane except in the few instances in which were false passage and the like. The following are his conclusions:

"1. Fibrous strictures of the urethra scarcely ever proceed from inodular (cicatricial) tissue. 2. False membranes are never found on the mucous surface of the canal. 3. Fungosities to which blennorrhagia has been attributed, can only be exceptional occurrences. 4. The mucous membrane of the urethra is never exclusively the seat of stricture, and in all the cases I have seen, stricture of this membrane was the consequence of a lesion situated externally to it. 5. In the great majority of cases, stricture is due to the retraction of the indurated fibres of the reticular tissue subjacent to the mucous membrane. The point of departure is often a deposit of plastic lymph. 6. In strictures exhibiting projecting knobs beneath the skin, incision from without inwards is the only means of obtaining a certain cure. 7. It is because stricture is situated beyond the mucous membrane, and involves the subjacent fibrous tissue, that surgeons who scarify the canal obtain in fibrous strictures less deeply placed than those mentioned above, cures which would be in vain sought from dilatation. 8. In gonorrhœa, the glandules of the urethra which extend obliquely within the substance of the mucous membrane, to a length of more than a *centimètre*, being filled with muco-pus, it is very improbable that a single injection would act throughout their whole extent. On the other hand, throwing in several caustic injections, one after the other, endangers the increase of the urethritis to a degree of intensity in which the inflammation becomes propagated to the subjacent reticular tissue. 9. The most certain means of preventing stricture consists in treating gonorrhœas that have become old, and no longer cause pain, by compression of the glandules, made by the successive passage of several bougies." (tome iv. p. 141.)

We subjoin an interesting extract concerning some of the consequences of the deposit of plastic lymph in the spongy tissue of the urethra:—

"If the existence of this plastic deposit is admitted, we are furnished with the explanation of a fact hitherto unexplained—I mean the frequency of stricture opposite the bulb. In fact, the meshes of the reticular tissue being so large at no part of the canal as at this point, it is here the inflammation should most readily extend from the mucous membrane, and here, consequently, ought the deposits of coagulable lymph be most frequently met with.

"When once the lymph is deposited in the meshes of the reticular tissue, it impedes the passage of the blood which goes and comes from the bulb to the glans, and this obstacle to the circulation, conjoined to the induration of the fibrous membrane, explains why persons who have suffered from severe gonorrhœa, find so much difficulty in excreting the last drops of urine. I have shown, in a memoir I presented to the Académie de Médecine, that the excretion of the semen and urine, in all that part of the urethra which is placed anteriorly to the bulb, is due to the pressure exerted upon the contents of the canal by a column of blood propelled from the bulb towards the glans by the contraction of the bulbo-cavernous muscle.

"This obstacle to the circulation also explains how, in old and well-marked stricture, the glans, as compared with the rest of the penis, acquires so greatly an increased development, that I have been able from this alone to form my diagnosis in a very large number of cases. When a deposit of lymph exists in the spongy portion, the blood propelled by the bulbo-cavernous muscle may still reach the glans, though with difficulty; but its return being obstructed, this portion of the penis becomes tumefied by the stagnation of blood, a portion of which may also become coagulated in the absence of the movement which maintains its fluidity." (p. 133.)

We pass over a memoir upon *Obstetrical Anæsthesia*, by M. Houzelot, and a Report upon the same by M. Laborie, both lamenting the little way the obstetrical employment of anæsthetic agents has made in France. Neither communicate anything upon the subject that is new to the English reader.

VII. *On the Hereditariness of Syphilis.* By M. Cullerier.

In this paper, the surgeon of the Lourcine attacks the generally received opinion, that constitutional syphilis can be communicated to the fœtus by the male as well as by the female parent. He long held the opinion that it could, and gave the usual caution to men about to marry, though exhibiting symptoms of constitutional syphilis, of the danger they ran in infecting their offspring. The numerous instances, however, in which perfectly healthy children have resulted from such marriages, have induced him to alter his opinions; and he appeals to the experience of all practitioners, in confirmation of his assertion that men, the subjects of secondary or tertiary symptoms years after their marriage, the consequence of early primary affections, yet procreate entirely healthy children, in whom no trace of syphilitic disease can be detected.

On the mother's side, syphilis may become hereditary under all circumstances; and when it has once entered her system, she may produce syphilitic children at all epochs of the evolution of the disease; and that whether she exhibits actual symptoms, or seems to enjoy good health in the intervals of the appearance of these. Interrogation of the mothers in special hospitals, in which syphilitic infants are born, proves that either they were ill at the time of delivery, that they had been so during their pregnancy, or that they had exhibited primary or constitutional symptoms a longer or shorter time prior to becoming pregnant; and could the

mothers in private practice be questioned in a similar way, there can be no doubt the same results would be arrived at.

So, too, were the cases of reputed communication of syphilis by the male parent subjected to a rigorous analysis, their number would undergo a singular reduction, and there would scarcely remain others than those in which, at the time of connection, a contagious affection, and consequently a possibility of infecting the mother, existed. It is upon the condition of the mother being so infected, that the possibility of the infant becoming so, entirely rests. Again, in cases in which repeated abortion has been supposed due to syphilitic taint, treatment of the mother is of avail, while directed to the father it is useless. One thing that renders tracing the history of these cases difficult, is that the fathers, while they willingly enough admit venereal accidents that may be referred to their youth, stoutly deny the existence of any contagious symptom that may have existed at the time of marriage.

M. Cullerier submits M. Ricord's views to a criticism they have long stood in need of, and shows upon what slight grounds this sarcastic critic of other men's views builds up his own theories. He appeals to other practitioners also for the results of their observations, pointing out, if his own views become confirmed, how consolatory they must prove as compared with those of Ricord; according to which, a man who has once had constitutional syphilis, is never certain that he is rid of it, and always continues in danger of procreating syphilitic children. As the number of syphilitic women is infinitely less than that of syphilitic men, especially in private practice, where such cases are quite exceptional, the presumption of hereditariness becomes restricted within very narrow limits; and the daily experience of every practitioner proves the great rarity of syphilitic births as compared with the prodigious number of individuals who have suffered from syphilis prior to becoming fathers.

After expressing his disbelief that several pathological appearances, such as suppuration of the thymus, hepatisation of the lung, pemphigus, &c., are really signs of hereditary syphilis, M. Cullerier makes the following practical observations:—

"It only requires some acquaintance with a hospital in which are lying-in women and new-born infants, to become convinced of the inexactitude of those authors who give as signs of hereditary syphilis the slight corporeal development, the miserable appearance, and wrinkled skin of these infants, giving them, according to Doublet, a resemblance to an old man, or causing them to appear shrivelled up and half boiled, *semi cocti*, in the words of Fallopius.

"In the immense majority of cases, the infant who has derived a syphilitic taint from the mother, when it has continued to live through the whole intra-uterine period, is born well-formed and in good health, and undergoes its normal development during two or three months. Toward this period, sometimes earlier, rarely later, general symptoms are manifested, the precursors of the venereal affection about to exhibit itself. The child sucks with less avidity, it sleeps badly, the bowels are disordered, the skin of the face assumes a bistre colour, and very soon unequivocal signs of lues appear. In most cases, mucous tubercles are observed, about the arms first, then on the genital organs, next in the folds of the skin, and on any parts of the body that become soiled with urine or fæces. At the same time that the tubercles become developed, spots appear on the skin of the chest and abdomen, which are nothing else than roseola, but in general so fugacious a roseola, that one does not always see the child soon enough to perceive it, while

also it is often taken for simple erythema, so frequently met with in sickly and ill-cared-for children. As the consequence of hereditariness, the other forms of syphilis are more rare. Thus, the papular form is quite exceptional. The pustular form is somewhat less so, and a variety of it, impetigo, is often met with upon the face and head of children who exhibit mucous tubercles or roseola. It is rather in the relapses of hereditary syphilis than in its primary manifestation, that those forms, lichen and ecthyma, are met with. Lesions of the osseous, fibrous, and cellular tissues are very rarely seen as early occurrences, although they are sometimes met with. But when syphilis has begun by the skin and mucous membranes, as is the usual case, it may afterwards invade these tissues, if its progress has not been arrested by treatment. . . . .

"All I wish to call to mind is, what every one knows very well, first, that it is extremely rare to find infants born with symptoms of syphilis; and next, that those in whom these will be exhibited at a later period, may offer, until this epoch, all the signs of good health and a good constitution; and especially if the mothers during their pregnancy have not fallen into a state of too advanced cachexia, if they have not become too much exhausted by moral emotions or physical privations, or by the treatment they have been submitted to." (tome iv. p. 257.)

After adverting to the remote period assigned by some authors at which hereditary syphilis may still appear, M. Cullerier observes—

"These questions are not always easy of determination, and they must remain very obscure when we have only the accounts of mothers and nurses to go by. But when we are able to observe these infants from the moment of their birth, we find that it is towards the age of six weeks, or of two or three months, that the first symptoms are seen. Sometimes, but rarely, it is not until the fifth or sixth month, and hardly ever so late as towards the end of the first year. Thus, if I were required to lay down a general rule as to the epoch of the appearance of hereditary syphilis, I should not hesitate to say that it is during the first twelve months of life that it manifests itself; and that when an infant, about whom we are uneasy, has completed a year without any manifestation whatever, we may regard it as indemnified from the disease it might have derived hereditarily. This precision is very different to the vagueness of the statements of most authors, and to that uncertainty which threatens the child during its whole life with the consequences of the faults or the misfortunes of its parents. Future experience will decide whether it is an exaggerated pretension." (p. 261.)

VIII. *On a New Mode of performing Lithotomy in the Female.* By M. Vallet.

In this paper M. Vallet, Senior-Surgeon of the Hôtel Dieu at Orleans, recommends a new mode of performing vesico-vaginal lithotomy, by which fistula, the ordinary sequel of the operation, may be avoided. It consists in making the incision into the bladder in a transverse direction, and proceeding at once, after the extraction of the calculus, to the application of sutures. Two cases are given in which he so operated. The patient is placed as in ordinary lithotomy, and the operator, seated opposite her upon rather a high chair, passes in a univalve speculum, which he confides to an assistant placed on his left, and who forcibly depresses the lower wall of the vagina; two other assistants keeping the labia stretched by the aid of bent levers or of their fingers. A catheter is introduced, and its extremity is directed towards the *bas-fond* of the bladder, so as to project the portion of the organ that is to be cut into. The incision extends over a space of three *centimètres*, commencing at the

middle part of, and external to a line drawn from the urethra to the ureter (representing one of the sides of the *trigone*), and reaching to the same point on the opposite side. Immediately after the calculus has been removed, some injections are thrown into the vagina, in case of hæmorrhage, which is rarely considerable, and then three or four points of suture are introduced, leaving rather less than a *centimètre* between each. The threads are tightened sufficiently to completely approximate the edges of the wound, and their ends cut off. A gum elastic, or gutta percha catheter is left in the bladder, care being taken that it does not come in contact with the sutures. Great care also is taken to keep the catheter clear for the passage of the urine. On the seventh day the patient is examined by aid of the speculum, and if the union appears firm the sutures are removed, the catheter being kept in somewhat longer.

In the author's first case, three points of suture were employed. They were removed on the seventh day, the catheter being left in until the twelfth. Although the patient at first discharged her urine involuntarily, as before the operation, in a few weeks she recovered the power over the bladder, and was discharged quite well. In the other case, one of the three points of suture was found to have cut through the edges of the wound, and a minute fistula remained, to remedy which the edges were pared, and another suture employed several weeks after, with complete success. Both these cases were reported to the Society two years after their occurrence.

For future operations, the author recommends an instrument he has devised, and tried with success on the dead subject, to facilitate the projection of the portion of the bladder to be divided, in order to render its incision more easy and more exact. It consists of a kind of grooved director, somewhat longer than the ordinary female catheter, which is flattened over the last fifth of its extent, where also a moveable branch, four *centimètres* long, is attached, and which, by a rotatory movement, can be brought into a transverse position, giving the director a cruciform appearance. Along this transverse branch the knife is to be carried when making the incision.

We must reserve the notice of the remaining two fasciculi of this volume for another opportunity.

Review XI.

Lectures on the Principles and Methods of Medical Observation and Research, for the use of Advanced Students and Junior Practitioners. By Thomas Laycock, M.D., F.R.S.E., F.R.C.P., Professor of the Practice of Medicine, and of Clinical Medicine, in the University of Edinburgh, &c. &c.—*Edinburgh*, 1856. Post 8vo, pp. 218.

The objects which Professor Laycock has proposed to himself in the preparation and publication of this volume, will be best set forth by the following extract from his Preface:—

"When about to enter for the first time upon his duties as Professor of Clinical Medicine, and to deliver the summer course of Clinical Lectures for 1856, in the

University of Edinburgh, the author looked about for some elementary work on the inductive philosophy which he could recommend to his class, for their instruction and guidance in clinical observation and research. He found several sufficiently able manuals of *physical diagnosis* adapted to students; and good elementary works on the uses of the microscope and on the *routine* of the clinical wards, with systematic instructions 'how and what to observe.' But he found none which instruct the medical student in a simple and easy form how to use his reason; none which explains to him in especial the nature of the mental processes by which knowledge is acquired in his particular sphere of labour; none which teaches him the applications to practical medicine of those aids to the intellectual powers which modern inductive philosophy uses so commonly and so efficiently. The student would inquire in vain for a short and practical exposition of the numerical method of research, in its special applications to practical medicine, or of that still more effective and philosophical method, the *analogical;* a method which, when once understood, is singularly easy of application, and equal (the writer is deeply convinced) to the solution of all the problems of life and organization that it is possible for the intellect of man to conceive, however profound they may be. A method, in short, of unlimited powers, and specially adapted to the needs of medical science."

After pointing out that the peculiarities of medical reasoning require a special direction to be given to the ordinary systems of logic, to adapt them to the needs of the medical inquirer, Professor Laycock continues:—

"Seeing this defect in medical literature, the writer determined to deliver to his class of Clinical Medicine a few lectures in which elementary principles and processes of observation and inquiry should be presented to it in as simple and attractive a form as possible, and as devoid of metaphysical phrases as they well might be made. He was not regardless, however, of the necessity that they should also be as practical as possible; that is, that they should be adapted to the actual position and wants of the student. It is with this object that simple illustrations are introduced; it is with this object, too, that the attention is directed rather to the observations of the processes of disease than of the products or results of these processes. . . . It further seemed to the author of some importance that the student's attention should be directed especially to the natural history of disease, with a view to a more philosophical, more really practical, and more truly natural system of medicine than has hitherto been given to the world. Hence the introduction of a subject not often noticed, and when noticed, always imperfectly, in systematic works,—the order of succession of vital phenomena." (Preface, pp. vii.—xiii.)

Fully agreeing with Dr. Laycock as to the fundamental importance of a well-trained reasoning faculty for real success in practising the art of medicine, still more in extending the boundaries of the science, we must express a general doubt whether such training can be effectually given by the study of books. To us it seems essential that the student should enter upon his medical course with a mind which already knows how to exercise itself aright; and this practical knowledge is acquired in early years (as we have had large opportunity of observing) far more certainly and effectually under the guidance of a judicious teacher, who sets his pupils to reason about "common things," and then makes them comprehend the *rationale* of the processes they have empirically gone through, than by the study of systematic treatises on the science of reasoning. In fact, however valuable the study of such treatises may be as an exercise to the mind, like that of classics or mathematics, we have a strong conviction that such study does not in itself engender any special capability

of dealing with the ordinary affairs of life, still more for resolving the intricate problems of physiology or medicine. For the forms of logic deal only with certain propositions, whose certainty is assumed (like that of the axioms of geometry) as essential to the reasoning process; whilst in reasoning about "common things," still more in medical reasoning, the degree of probability which attaches to the data is one of the most difficult parts of the inquiry, which pure logic gives no assistance in solving. And thus we have lately had to witness the humiliating spectacle of one of the greatest mathematicians and logicians of the age entirely "possessed" by the delusions of table-turning, spirit-rapping, and the like, just because he was not capable of applying his formulæ to the detection of the fallacies which lie at the root of the whole system.

To a mind which has been already trained in the art of reasoning, and from the art has been led upwards to know something of the science, it must be most useful, in the prosecution of his medical studies, to be made to perceive in what the special peculiarities of the living system consist, and what modifications of his pre-formed habits of thoughts are required in his new vocation. But this, again, cannot, in our opinion, be taught in abstract propositions, by any means so effectually as by a course of practical instruction. It is the proper vocation of the physiological teacher to unfold to his pupils step by step the mysteries of organization, to show them how to apply the great doctrines of causation to the actions of the living body, and to lead them to work in their own minds such simple problems, as may serve for an introduction to the more complicated. And by the time the student enters on his clinical course, he ought to be well prepared to reason upon the phenomena of disease, to bring to bear upon the consideration of them not only the knowledge of healthy action, but an acquaintance with its conditions, and thus to search out the latent source of morbid phenomena, and to devise means for its rectification. In guiding his pupils in such a course, the clinical professor, like those who are engaged in the antecedent training of the student, should trust more to example than to precept. A *clinical* lecture ought surely not to be a disquisition upon abstract principles, but upon the interpretation of phenomena; and in the various processes which this interpretation involves, there is such a wide scope for the exercise of *all* the intellectual processes, that it must be the fault of the teacher if he do not find abundant opportunities of pointing out the application of the fundamental principles of reasoning to the science and art of medicine. But if the student have not had some such preliminary training as we have described, he will scarcely be likely to profit by even the very best clinical instruction to more than a limited extent; and the clinical professor must feel that he wastes his time in endeavouring to teach men how to reason upon one of the most intricate of all subjects, who do not know how to reason upon the most simple.

In all this we dare say that Professor Laycock would fully concur with us; and we shall presume that he intends his present publication merely to supply, so far as it may, the deficiency which he has experienced, without supposing that it will serve as a royal road to the art of medical reasoning, for those who have never been trained to reason well about common things. We have endeavoured carefully to estimate its value

from this point of view; and with every wish to do the author full justice, we are compelled to say that his treatise is far from coming up to our idea of what might reasonably be expected from the successor of the Cullens, the Gregories, and the Alisons. We believe that Professor Laycock is capable of much better things, if he will only give himself time, and will thoroughly digest and systematize his abstract views, giving due heed to other men's knowledge and experience as well as to his own. The table of contents at once strikes us as rather a catalogue of hastily-concocted fragments, than as a bill of fare of a well-arranged banquet; and this impression is not dissipated but strengthened, as we advance through the pages to which it introduces us. But we should be doing injustice to its able author if we did not, at the same time, give him credit for the many acute suggestions which are scattered through the volume, and for his clear-sighted exposure of many prevalent fallacies. In fact, he seems to us to have succeeded much better in showing what course is to be avoided, than what is to be followed; and his book is, consequently, more likely to do good by teaching its readers what *not to do*, than what *to do*. In the present state of medical art and science, however, this is a lesson which every one ought early to learn. For if ever there be a case in which the system of the circumlocution office—"how not to do it,"—may be rightly carried into action, it is in the avoidance of the temptation to the mischievous routine of empirical practice, which satisfies both doctor and patient that "something active" is being done, the said "something" being too often about the last thing in the world that ought to be done.

To these general remarks, we shall now subjoin a few extracts, which may serve to give an idea of the merits and demerits of Dr. Laycock's treatise.

One of the most common of all errors in medical reasoning is the substitution of a theory for a fact—a probability for an actuality:—

"This is so common an error, that you can hardly open any medical essay without meeting with it; and it is a very insidious error. There are two or three principal modes in which it occurs. One is the use of collective words or general terms, as facts, which are essentially theoretical—as 'tonic,' 'diaphoretic,' 'fever,' 'inflammation,' and the like. In the search after accuracy, a definition is given, and then it is thought that the word has a definite meaning. This is not so, however. Naturalists have not even defined what a species is, although they have only objective phenomena to deal with, and can place the objects before them. In defining processes and states of living bodies, we must remember that we do not know the entirety of any one process, for we have never observed it—sometimes, indeed, only a small portion of a process; as to the remainder, we draw conclusions only, that is, establish theories or probabilities. Nothing is so difficult to handle in this way as the phenomena of life, because all vital phenomena are continuous, or run into each other. It is this continuity that renders it impossible to define a species with absolute strictness, or even what is animal and what vegetable. All words and terms in medicine, then, are sources of fallacy." (p. 23.)

A few pages further on, we meet with the following apposite illustration:—

"It is asked, is cholera an infectious disease? can cholera be communicated? in the most perfect good faith, and without the slightest suspicion that in the use of the collective term itself, there is a fundamental fallacy. Cholera is but a word by which a group of symptoms is indicated; it is not a thing. The questioner

meant, therefore, to ask this question—Can the *materies morbi*, the cause of the symptoms, be generated in the bodies of the sick, and communicated to the bodies of the healthy, so that in them a similar disease or group of symptoms will result? Now it has in fact been assumed, without due inquiry, that the group of symptoms designated by cholera are the only phenomena which resulted from the *materies morbi*; so that during the whole of the first epidemic at least, it was hardly guessed that, etiologically, the immensely greater number of cases of diarrhœa were cases of slight cholera, that is, due to the same cause. I need only add, that there was still less suspicion, that in such slight cases the *materies morbi* might be generated and given off equally, as in the more intense forms." (p. 41.)

Now although we are ourselves inclined to accept this view of the case, we cannot adopt it with the unhesitating confidence that the author himself expresses in its truth, as if it were a point now universally conceded. Has he not been guilty of the very fallacy which he exposes in others; and is not his assumption of a *materies morbi*, transmitted from one individual to another, just as hypothetical as the limitation of the designation cholera to the ostensible cases of that disease? Our next extract will show how strong is the trust placed by Dr. Laycock in his own theory of contagion, in a case to which the profession generally would think it utterly inapplicable:—

"Another illustration of the fallacious substitution of a theory for a fact in causation, is afforded in the current theory of the rapid spread of the epidemic poison which excites the group of phenomena termed influenza. It is highly characteristic of a strictly infectious agent; and but for the circumstance that an insidious unperceived theory takes the place of fact in the consideration of the question, would suffice to establish its infectious character. So soon as the mind addresses itself to the question of causation, it instinctively compares the spread of influenza with its knowledge of the mode of the spread of other epidemic diseases that are known to be infectious, as the exanthemata. But it finds that the identical characteristic of influenza, which to an unbiassed judgment would most strikingly indicate its infectious character—namely, its rapidity of spread—is wholly different from these. They require many months to infect an entire population; influenza never requires as many weeks. This dissimilarity being noted, and no other facts as to influenza being brought into comparison, the inference is drawn, that the diseases are really dissimilar as to the contagious element, and that the influenza spreads too rapidly to be caused, like them, by an infectious agent. Then, as the mind cannot rest satisfied without a cause being assigned for the rapid spread, the agent nearest the apprehension—namely, the atmosphere—is selected, and so it is concluded that influenza spreads in consequence of some change or unusual 'influence' in the atmosphere. This false theory of epidemic causation is not peculiar to influenza; it is the most common, as well as the most mischievous of the epidemiological fallacies. Since the days of Sydenham, who in modern times gave it extended currency under the term 'epidemic constitution,' it has obstructed our progress in ascertaining the true nature of epidemic diseases, and has been erroneously applied equally to cholera, plague, yellow-fever, as to influenza." (p. 43.)

There is an old proverb, that "those who live in glass houses should not throw stones;" and Dr. Laycock, in thus attacking the doctrine which derives its sanction from the accumulation of a large mass of carefully-observed phenomena, and which has been regarded by men not inferior to himself as the hypothesis best fitted to account for the facts of the case, must not be surprised if he finds his own doctrine—which, while fully as hypothetical, is based on a far more limited induction—scouted as absurd by the united voice of the profession. To our own minds nothing

is more clear than that the phenomena of the spread of influenza are totally inexplicable by any form of the doctrine that the reproduction of the poison in the living human body, and its transmission by contact, are necessary conditions of its epidemic diffusion; and though we would not exclude contagion as a possible means by which it has been occasionally conveyed whither it might not otherwise have reached, yet of all diseases of the zymotic class, we believe that influenza is one of the least contagious. And the striking modification in the type of *other* diseases, which has been observed by intelligent practitioners in the metropolis and elsewhere, during the prevalence of influenza, has always appeared to us one of the most striking illustrations of the truth of Sydenham's doctrine of the influence of "epidemic constitution." We admit that the term is hypothetical; but so is the term "contagion" in every instance in which we cannot prove the direct transmission of a *materies morbi;* and the question is, which hypothesis is best founded on induction from facts?

We give the following paragraphs from the fifth lecture, "On the Due Estimate of Treatment, and on the Management of the Case," as examples of the practical wisdom with which this part of the work abounds:—

"*Estimate of Probabilities.*—This, perhaps, is the department of medicine in which the most profound sagacity may be shown. Practical medicine is confessedly a conjectural art; to conjecture wisely is therefore the essence of the art—the whole stuff and pith. The numerical method affords us a numerical estimate of the probabilities in a given number of cases; but this is not of much help to the practitioner at the bedside, who has to determine the probabilities in the individual case before him, and which may or may not be more or less similar to the cases estimated numerically. What, in fact, is essential to this sagacity in especial, is essential to form the whole man as a practitioner. He must have the habit of minute and accurate observation, so as to be able quickly to detect all possible circumstances that can throw light upon the case; he must have sound experience, so that he can compare what he now sees with the results of that experience; and he must have knowledge, that he may correct and extend his observations, and correct and simplify his conclusions. Nothing but constant and painstaking exercise of the faculties necessary to these mental processes, can give the requisite quickness of perception, comparison, and deduction. But in proportion as these faculties are possessed by the practitioner, as well as in proportion as they are exercised, will he be sagacious in his estimate of probabilities. Some men are wholly unfit, naturally, for the exercise of the art, simply because they want the requisite faculties of mind; some because they want the requisite industry." (pp. 144—146.)

"*Error of Impatient Expectation of the Results of Treatment.*—When a practitioner has clearly laid down his plan of treatment, he must carry it out steadily, and patiently await the result. Nothing is so detrimental to success in treatment, as an indefinite conception of the end to be attained, or of the means to be used. The practitioner so situate is constantly vacillating; being swayed by every change of symptom in the patient, by every expression of opinion he may hear. He is, therefore, constantly changing his remedies or method of treatment. Thereby he renders the symptomatology more confused by superadding the varied phenomena induced by drugs to those of the original affection, and thus at last his diagnosis is utterly bad. In consultation-cases in which treatment has been long continued, or, if not long continued, has been actively pursued, the first step in the examination is a careful separation of the results of this kind of treatment from the results of disease. Thus you may be called into a case in which the starvation-system of certain homœopathists [and of many allopathists also] has been rigidly carried out. You may be told it is gastric fever; but in reality the patient is perishing for want of food and drink. In cases of intestinal obstruction, it is not an unfrequent

circumstance to find the patient suffering more from the effects of drastic purgatives incautiously taken or wildly administered, than from the primary disease. It is a great point in your art to know when to do nothing, and to be able firmly to resist all solicitations to be very actively doing something. A clear conception of the case in all its bearings can only [alone?] give you this admirable quality. And for this, therefore, you must incessantly labour, never being content until you have exhausted every available source of the knowledge that can help you in your diagnosis." (pp. 146, 147.)

Per contra, when we turn to the last lecture, on the Analogical, Philosophical, or purely Inductive Method of Research, we experience little save disappointment at the very imperfect view of medical philosophy which it contains. After the grandiloquent panegyric on the "analogical method," in the preface, as "equal to the solution of all the problems of life and organization that it is possible for the intellect of man to conceive," we were quite unprepared to find our author dismissing it after so summary a fashion, as if he was tired of his subject and wanted to have done with it. He informs us that "the primary or fundamental principle of life is the unity of structure and function of organisms both in time and space" (p. 181); and he tells us (p. 189) that it is by the application of this principle that we are to distinguish true analogies from false ones. The greater part of the lecture is taken up with the illustrative application of this analogical method, in the investigation of the pathology of bronzed skin, and of gout and rheumatism; in which Professor Laycock doubtless shows great acumen, but at the same time lays himself widely open to criticism, both as to the soundness of his data and the justice of his conclusions. Our space, however, does not allow us to follow him through these inquiries; and we must conclude by expressing the opinion that, with much that is sound and good, the book contains much that is questionable (to say the least), and would have probably attained a far more perfect development, had its period of incubation been two or three years instead of two or three months.

Review XII.

The Australian Medical Journal. Edited under the Superintendence of the Medical Society of Victoria.—*Melbourne.* No. 1, January, 1856. pp. 180. No. 2, April, 1856. pp. 160.

The appearance of a quarterly Medical Journal of considerable merit, in a district which but few years back was unknown and uninhabited, is no unimportant sign of the times. The editors observe in their opening number, that—

"The temporary disorganization which succeeded the discovery of a new source of material wealth, has gradually subsided, and the colony of Victoria now occupies, in its social and domestic characteristics, a position unsurpassed by any dependency of the British Crown. Every element of future greatness and national progression has received a powerful acceleration during the past three years, and the self-adjusting principles upon which civilized society is constructed has operated insensibly, but with unerring certainty, in restoring the equilibrium briefly disturbed by a sudden interruption of the tranquil and industrious avocations of the people. With an immense influx of population, higher tastes, superior talents, and elevated desires have accompanied other acquisitions; and the press, at once the representative and the auxiliary of public sentiment, has fully sustained,

by its progress and improvement, the high estimate which free countries entertain of its power and utility. Politics, literature, and law, have their representatives; agriculture, horticulture, science, and religion, possess their recognised exponents; and even the humourist has his special vehicle of thought. The medical profession alone seems to have been left destitute of an accredited organ to maintain and fortify its proper position in the social fabric, of which it constitutes an important support. We need not, therefore, enlarge upon the advantages to be derived from the establishment of a medical journal."

These advantages, manifest as they are under all circumstances, where hitherto no medium of intercommunication has existed for the members of a liberal profession, are peculiarly striking under the relations that present themselves in Australia. We there have a rapidly growing and wealthy population, an amount of intellect in every way commensurate with the vigour that marks all the branches of the great Anglo-Saxon tree, and a soil in which there have as yet been no cultivators, where all the experience of the past may be brought to bear without any of the drawbacks incident to the slower growth and development of the older countries. The editors of the *Australian Medical Journal* have indeed a glorious field before them! That they may be fully conscious of the great responsibilities that rest upon them; that, as the guardians of the ethics of the profession, they may keep the loftiest goal before their eyes and pursue it;—that they may rise superior to the temptations that beset the journalist, and scorn petty jealousies, seeking only the advancement of science and the good of the common profession,—such are the wishes we offer to our friends in the young but stalwart colony. They have entered into their new path with a vigour which justifies fair hopes; and none will rejoice more than their brethren on this side of the globe to see those hopes realized.

The information contained in the Journal is distributed under the following heads:—Original Contributions; Hospital Reports; Reports of Medical and Scientific Societies; Editorial Articles; Reviews; Extracts from Medical and Scientific Literature; General Correspondence, Medical News, and Answers to Correspondents.

We may find an opportunity of giving an analysis of one or more of the papers. We subjoin the titles of the communications contained in the first number, which cannot fail to interest our readers,—viz., 1. On the Principles of Pathology, by W. B. Wilmot, M.D. 2. Sunstroke, its Causes, Consequences, and Pathology, by C. Travers Mackin. 3. On Barbiers, by Richard Eades, M.B. 4. Case of Autumnal Fever, with Remarks, by R. T. Tracy, M.D. 5. Epilepsy produced by Pressure on the Brain, by John Maund, M.D. 6. A Cure of Recto-labial Fistula, by Edward Barker, Senior Surgeon to the Melbourne Hospital. 7. On Lloyd's New Operation for Stone, by W. Gillbee, Honorary Surgeon to the Melbourne Hospital. 8. Case of Supposed Rupture of the Uterus, with Spontaneous Evolution of the Fœtus and Placenta Prævia, by G. S. D. Butler, M.D.

The mere enumeration of the papers certainly does not convey much to our readers, but it serves to indicate the path which our Australian colleagues have entered upon. Their own prowess will secure their future career; but we trust they will not refuse to accept from us the cordial assurances of the sincere sympathy and good fellowship, with which their first essays will be hailed in the mother country.

PART SECOND.

Bibliographical Record.

ART. I.—*A Practical Treatise on the Disorders of the Stomach with Fermentation.* By JAMES TURNBULL, M.D., Physician to the Liverpool Royal Infirmary. *London*, 1856. pp. 160.

IN the first chapter, Dr. Turnbull examines very briefly the process of natural digestion, stating the arguments used to support the idea that the conversion of food into chyme is a fermentation of a peculiar kind. In the second chapter, he discusses the chemistry of the changes involved in the alcoholic, the saccharine, the viscous, the lactic, the acetic, the gallic, the pectic, the benzoilic, the sinapic, the ammoniacal, the putrid, and the fatty fermentations. It must be confessed, however, that science is in the rear of what practical observation requires on these heads, and consequently only the most important of them are discussed in detail. In some, if not all fermentations, the growth of organic forms of an elementary nature, such as the yeast-plant, is looked upon by the author as an important part of the process. The action of certain chemical agents in arresting fermentation is pointed out; such as bichloride of mercury, sulphate of copper, carbonate of soda, mustard, wood spirit, creasote; others, such as turpentine, chloride of lime, weakened but did not destroy it; tannin precipitated the ferment, but without arresting fermentation; and gallic acid, on the other hand, caused an abundant dark head. Experiments were likewise made by the author upon lactic fermentation, which, however, none of the reagents mentioned, except wood spirit, had the power of completely checking. It may be remarked that the arrest of fermentation is not exactly the same thing as arrest of putrefaction; for arsenious acid, so decided an antiseptic, has no great power in checking vinous fermentation. The third chapter commences the medical part of the work, and is devoted to the analysis of 33 already published cases of *sarcina ventriculi*, to which Dr. Turnbull adds 3 of his own. As might have been anticipated from the introductory chapters, he considers this organic growth as having more causal influence upon the morbid phenomena, than is usual among pathologists. He thinks it is produced by, and also propagates, a morbid fermentation. The palliative effect of hyposulphite of soda, and of alkalies, he attributes to the partial arrest of fermentation which they exerted in his experiments; and seeing the complete arrest effected by wood spirit, he advises a trial of that as a remedy for *sarcina*. The next chapter is occupied by "Dyspeptic disorder" with

morbid fermentation but without *sarcina*, and in both stomach and bowels. In this Dr. Turnbull expands upon the narrow view of the process of digestion which his title would have seemed to imply, and carries on the arguments which had been applied to the stomach into the further regions of the duodenum, ileum, and colon. He attributes English autumnal cholera to the acid fermentation of fruit in the bowels. Bichloride of mercury having been shown in the second chapter to be an anti-fermentive, "not only explains," he says, "how mercurials act in these disorders of the bowels, but may also throw some light on the manner in which mercury acts as the most energetic remedy in eradicating syphilis." The fifth chapter examines the action of other remedies on similar principles. The sixth enumerates the various tendencies of different articles of diet to ferment, and explains their value as aliments. In order to feel the force of the author's reasoning, it is necessary to agree with him that a vast number of organic acts are explained by referring them to "fermentation" —that not only cookery and digestion, but that indigestion also, diarrhœa, colic, flatulence, syphilis, the poisoning by bitter almonds and by German sausages, the acidification and the alkalinisation of urine, the growth of sarcina, and the pain in catarrh of the stomach, have their nature elucidated by ascribing them to this word.

ART. II.—*Nomos: an Attempt to Demonstrate a Central Physical Law in Nature.—London*, 1856. 12mo, pp. 198.

IN the year 1842, Mr. W. R. Grove, in a course of lectures delivered at the London Institution, propounded a theory of the correlation of physical forces, which subsequently assumed this expression, "that the various affections of matter which constitute the main objects of experimental physics—viz., heat, light, electricity, magnetism, chemical affinity, and motion, are all correlative, or have a reciprocal dependence. That neither, taken abstractedly, can be said to be the essential or the proximate cause of the others, but that either may, as a force, produce the others ... each merging itself as the force it produces becomes developed; and that the same must hold good of other forces, it being an irresistible inference that a force cannot originate otherwise than by generation from some antecedent force or forces."

In investigating the relations of the different forces, Mr. Grove commenced with each in its turn, and indicated the manner in which it could mediately or immediately produce the others. The clearness with which this correlation was shown, led "many to regard all the different natural agencies as reducible to unity, and as resulting from one force which is the efficient cause of all the others."* But Mr. Grove adhered to the simple expression of correlation, urging that "in tracing any force back to its antecedents, we are merged in an infinity of changing forms of force;" and in reply to the question, "Can we suggest a proposition, definitely conceivable by the mind, of force without antecedent force?" states, "*I* cannot, without calling for the interposition of creative power."

* The Correlation of Physical Force. Second Edition. 1850.

The conclusion at which the author of 'Nomos' arrives, is, "in short, that the inorganic world is ruled by one single law, of whose operation the phenomena of electricity, magnetism, light, heat, chemical action, and motion are only so many signs—the law, that is to say, of the laboratory; and that no secret in the world of inorganic nature can be fully understood except upon this assumption."

But even with this "assumption," we fear there are many "secrets in the world of inorganic nature" which cannot be "fully understood." But further, what this "law of the laboratory" is, we are at some loss to comprehend. The term is used "to express that central law to which the philosophy of the laboratory appears to point;" which, gathered from the whole of the work before us, is nothing more than the correlation of forces as stated by Mr. Grove; but which receives some definition at the conclusion of the survey of electricity—viz., "an action of duality, out of which arise, under peculiar circumstances, certain marked movements—an action which depends not upon incomprehensible imponderables, but upon certain definite and comprehensible properties of matter."

The author proceeds then to show that this law "dominates in nature," and that it "may be a cosmical law." The first application of it to the movements of the earth requires two assumptions—*e.g.*, "Let us assume that currents of electricity surround the earth in a direction which is parallel to the plane of the ecliptic; let us assume that similar currents proceed from the sun to the earth, and emerge upon the part which is nearest to the sun; and we may soon see that the earth must move around the sun, and that she must rotate upon her axis as well as move onwards in her orbit."

The old "cookery books" usually commenced their directions in regard to culinary ichthyology with the most important advice, "first catch your fish;" but the author of 'Nomos' assumes that his fish are caught, and proceeds to garnish his empty dishes in an extremely pleasing, but scarcely satisfactory manner.

The "law of the laboratory" comes to the rescue of distressed comets; but here, again, there are "assumptions," honestly admitted to be such, too numerous for our patience to count.

The "law of the laboratory" is brought to explain "some of the phenomena of natural heat;" which is represented as nothing more than a current of any kind under difficulties. Here we have the tides explained by expansion of the solid substance of the earth; an expansion which does not appear to affect the water, and which expansion is due to the heat of the moon (!) being focussed within the globe itself. The conclusion with regard to heat is, that "natural heat" (*i.e.*, heat outside the laboratory) is identical with "artificial heat" (*i.e.*, heat produced in the laboratory, but which we should have supposed to be as natural as the other). And what this has to do with the "central law," we leave our readers to divine; and also how "the law provisionally named the law of the laboratory," renders these secrets "*fully* understood."

Then, after two delightfully short chapters, each occupying about one quarter of a page, we pass to the conclusion of the whole matter as already stated. The author starts by defining "imponderable agents" thus—"Agents, that is to say, which are quite beyond the scope of physical

inquiry;" and he concludes with an "assumption." The reader, however, although he will probably have no more definite idea of a "central law" when he has finished the book than he had when he commenced, will find much to interest him in the perusal; and will find abundant and apt illustrations of the law of correlation of forces, which is at present the most general expression that can be scientifically employed with regard to them.

ART. III.—*The Life of Henry Cornelius Agrippa von Nettesheim, Doctor and Knight, commonly known as a Magician.* By HENRY MORLEY, Author of 'Palissy the Potter,' 'Jerome Cardan,' &c.—*London*, 1856. Two vols., pp. 304, pp. 332.

IN the fermentation preceding and accompanying the great events of the first half of the sixteenth century, Cornelius Agrippa occupied a position which fully justifies the choice of Mr. Morley in selecting his biography as illustrative of the times he lived in. In a purely medical point of view we learn but little from the work, but all who are interested in what bears upon the development of the human mind, will read the account of Agrippa's struggles and the analysis of his works with sympathy. In the middle ages, when natural science was mystified by the alchemist and by an admixture with a cabalistic theology, it is not surprising that a man who "began his life by mastering nearly the whole circle of the sciences and arts," should describe "Physic as another art of homicide, mechanical, though claiming the name of a philosophy."

The biographer offers no temptations to the present generation to follow in the footsteps of Agrippa, so that it is needless to warn the medical reader against the influence of the scholar's scepticism—the very crudity of which is a sufficient antidote against the poison he might have infused in his own day.

It is out of our province to inquire more fully into the features that characterize the doings or the writings of the great Magician, but those who desire an analysis of them, and who wish to know how they were received by his contemporaries, will find Mr. Morley's volumes an acceptable addition to their libraries.

ART IV.—*A Comparative Inquiry as to the Preventive and Curative Influence of the Climate of Pau, and of Montpellier, Hyères, Nice, Rome, Pisa, Florence, Naples, Biarritz, &c., on Health and Disease; with a description of the Watering-places of the Pyrenees, and of the Virtues of their respective Mineral Sources.* By ALEX. TAYLOR, M.D., F.R.S.E., Cor. Member of the Hist. Institute of France, &c. &c. A new edition, considerably altered and enlarged.—*London*, 1856. pp. 355.

THE plan followed by Dr. Taylor in the well-written book before us, is one that offers a better prospect of enabling us to determine the curative influence of climate, than the results which we may obtain by having regard to meteorological data only. He considers, justly, that we obtain a safe guide as to the effects of a given climate, by ascertaining the influence it exerts upon the native population.

"Thus, if in any climate we find that its agency is decidedly of a sedative kind, and that it proximately acts by modifying the tone of organs, we would, *à priori*, infer that such a climate would be unsuitable to that kind of diseased action depending upon general want of tone and a low state of functional energy. But, again, if in any climate we find that acute inflammatory affections—for instance, of the mucous membranes of the stomach or air passages—are a common disease with the natives, it would not seem to be a wise or logical proceeding on the part of a physician, to send to such a climate a stranger who was liable to be easily affected by these very maladies."

Regarded in their influence upon the invalid, Dr. Taylor classes climates as exciting, sedative, and relaxing: Nice, Naples, Montpellier, and Florence belonging to the first; Rome and, *par excellence*, Pau to the second category; Pisa and Madeira being instances of the third. Pau and Montpellier may be said to differ as much as, in our country, Torquay and Margate; no one would think of sending patients indiscriminately to these two places, and it is equally important in recommending residence abroad, that we should carefully distinguish between the localities, as we should between a stimulant or a narcotic medicine. The climate of Pau, to which Dr. Taylor devotes his chief attention, is characterized by the stillness of the atmosphere, the absence of free communicable humidity and of sudden transitions from heat to cold. Such conditions enable the invalid, without risk of sudden chills or the disturbance of the function of any organ, to enjoy out-door exercise throughout the winter, and amply account for the remarkable immunity from disease, and the longevity, of the inhabitants of Pau. The sedative action of the climate of Pau is shown by its physiological influences, and by the influence it exerts upon the *morale*. The inhabitants show a slower circulation, and are more phlegmatic than their countrymen generally; and visitors equally exhibit, after a time, a permanent reduction of their pulse and a modification of temperament. We are unable to give as full an analysis of Dr. Taylor's work as we might wish, but our readers will anticipate, even from what we have said, that the remedial action of Pau "may be summed up in one general principle: viz., wherever disease depends upon increased nervous and arterial action permanently produced, either by temperament or by some causes leading to more active disease," there we may expect a beneficial influence from this climate.

We are tempted to add a few words concerning a place which has been known for some years to the sojourners at Pau, but has of late acquired an extensive reputation, owing to the Emperor and Empress of the French having resorted to it—we mean Biarritz. It is a fishing hamlet about five miles from Bayonne, first brought into notice by the English residents at Pau, who went there during the summer to enjoy the invigorating sea breezes.

"It is most irregularly built, some parts of it being situated on a succession of cliffs, others in a species of ravine. The houses, being mostly intended for summer use, have all, more or less, a north-western aspect. And this is a matter of great importance, for in a southern climate, when a house has a southern exposure, its inhabitants are condemned during the day to darkness, for the admission of the sun's rays into the house would be intolerable; whereas, with a north-western aspect, the houses receive through their open windows the refreshing sea-breeze from the west, which most generally sets in every morning about ten o'clock, and the snuffing in of which is a real luxury, and goes far to neutralise the effects of a southern sun.

"The health of the native population at Biarritz is most satisfactory, and the advanced ages which they attain, as evidenced by the registers and the inscriptions on the tombstones in their romantic churchyard, prove how favourable the climate is; even with the drawback of hard work, the accidents of the sea, and of food not of the most fortifying and nutritive description."

Biarritz may be regarded as auxiliary to Pau; the warmness of the atmosphere and of the sea enable delicate individuals to bathe with benefit, who, in a northern climate, could do so only with prejudice to their health; at the same time it offers an agreeable retreat in summer from the sultry heats of Pau.

In concluding this brief notice of Dr. Taylor's book we would add that, as a scientific guide and a pleasant companion to the South of France and the Pyrenees, it will prove of great value to the physician at home, or to the invalid in quest of health.

ART. V.—*Addresses to Medical Students. Delivered at the instance of the Edinburgh Missionary Society.—Edinburgh,* 1856. pp. 266.

AMONG the dangers that beset the student of medicine when he first enters upon his career, none is probably more serious, or fraught with more pernicious results, than the temptation to employ the scraps of knowledge that he picks up, as arguments against the truths of Christianity. The ardour of youth, the fascinations of a new field of knowledge, the pleasure excited by a sense of emancipation from all intellectual control, conspire to lead away many a young man into the maze of infidelity, which is closely allied to the slough of immorality. Such is not a necessary result of the admission of the acolyte to the portals of science; but the danger is great, and the temptation is one too often yielded to. It is more necessary, perhaps, to the student of medicine than to the student of other professions, that his steps should be guided by men whose larger experience and more intimate knowledge of the relations that science and religion bear to one another enable them to assist him; and we therefore hail with satisfaction any attempt made with the view to showing the young student how to pass through the difficulties that environ him, how to reconcile facts which, to his crude mode of reasoning, appear subversive of religion. The addresses before us, like the addresses of the Christian Medical Association of London, are intended not only to promote religious sentiment among the students, but to show how their calling is hallowed, and their labours become more beneficial to themselves and to their patients, if they mirror themselves by the light of Christianity. We sincerely trust that the good which is intended by such associations may be fully realized; that they may tend to advance the best interests of the community, and to promote that charitable feeling which may be said to be a characteristic feature of the members of our profession, whatever the faults otherwise laid to our charge.

Of the addresses individually, we would speak generally in terms of praise; but to him who would hear the voice of the wise and good physician, the man of large heart and profound knowledge, on the momentous question of the reality of Christianity, and of its influence

upon humanity, to him we would especially recommend the prefatory essay by Dr. Alison. The University of Edinburgh, and other schools of medicine, would do well to secure the distribution of such papers as the one written by the Nestor of medical science, among those who are entering upon their studentship.

ART. VI.—*Military Sanatoria. Letter addressed to H.R.H. the Duke of Cambridge, Commander-in-Chief, &c., on the Introduction of Mineral-Water Establishments for the Use of the Army.* By P. PINCOFFS, M.D., M.R.C.P., &c., late Civil Physician to the Scutari Hospital. —*London,* 1856. pp. 37.

IN the October number of this Review we adverted to the fact that the establishment of military sanatoria had been suggested by Dr. Pincoffs, and was under the consideration of Government. In the pamphlet before us, Dr. Pincoffs explains in a lucid and succinct manner the grounds upon which the recommendation has been made, and the advantages to be derived from its adoption. In France and Germany, lengthened experience has shown that many soldiers whose constitutions are undermined by the wear and tear inseparable from military duty, are often, within a very short period, restored to health, by being sent to the sanatoria established in thermal and other watering-places, after having been treated for months to little or no purpose in the excellent hospitals of their various stations. Numerous arguments of a philanthropic character may be adduced for following the example set by continental nations, but none is so likely to be of weight with those to whom the soldier's welfare is immediately entrusted, than the fact that France, with a standing army nearly three times as large as that of England, should have fewer out-pensioners. On the 1st of August, 1856, there were 51,530 pensioners in the French army (including about 3000 in the Hôtel des Invalides), whilst our own pensioners amounted to 59,987.

"With all due allowances for the differences of the two services, owing to the conditions under which enlistment takes place, the length of service required, the vicissitudes of climate to which our soldiers are especially exposed, I do not hesitate to say that the comparatively small number of pensioners in the French army may in a great measure be accounted for, by the timely use and decided benefit derived from their thermal establishments; whilst the larger proportion of our pensioners is chiefly owing to the system hitherto pursued, of retaining a man in foreign service until his constitution is entirely broken, and then discharging and pensioning him; whereas he would have a fair chance of recovering his health and returning to duty were he sent home in time to such sanatoria as I propose."

We cordially recommend the perusal of Dr. Pincoffs' pamphlet to all who have the welfare of the soldier at heart; as it appears that the movement which he has initiated may prove successful (for we learn that a small experiment is already being carried out upon his principle at Bath), we trust it may react upon the civil sanatoria, infirmaries and convalescent institutions already existing, but often languishing for want of funds because the public are not sufficiently acquainted with the purport and importance of such establishments.

ART. VII.—*On the Composition of Food, and how it is Adulterated; with Practical Directions for its Analysis.* By W. MARCET, M.D., F.C.S., Licentiate of the Royal College of Physicians, Assistant-Physician and Lecturer on Physiological and Pathological Chemistry to the Westminster Hospital; formerly President of the Edinburgh Medical Society; Corresponding Member of the Société de Biologie of Paris, and Société de Médecine of Geneva, &c.—*London*, 1856. pp. 178.

WHEN we introduced Dr. Hassall's important work, 'On Food, and its Adulterations,' to the notice of our readers, we suggested that an abridged work of the kind would prove very acceptable to a large class of persons. The book which Dr. Marcet now offers is a substitute for the one we anticipated seeing from Dr. Hassall's pen. It is in a great measure, and confessedly, based upon the researches of the Analytical Commission of the 'Lancet,' and of other inquirers in the same field of science, while the author gives plain and simple directions for the methods most suitable for the detection of the various adulterations that are commonly practised.

We are glad to perceive that Dr. Marcet discourages the prevailing tendency to see "death in every pot," while he tells us how we may readily discover the fraud that tends to increase our bills and to diminish the rational enjoyment of our meals. Now that our tradespeople must know how easily they are found out, we trust that, for their own sakes as well as ours, they will universally appreciate the maxim of honesty being the soundest policy.

Dr. Marcet divides the whole subject into seven chapters, which successively treat: 1. On farinaceous and saccharine food. 2. On Spices. 3. On infusion of vegetable substances, other fluid vegetable food, tea, coffee, sauces, olive oil. 4. On fermented beverages. 5. On animal food, meat, fish, milk. 6. On mineral or inorganic food, salt, and water. And 7. On preserved food.

The descriptions of the substances are clear, the details of the microscopical and chemical analyses are succinctly and intelligibly given, and the illustrations are executed with that care which characterizes the productions of Mr. Lens Aldous and Mr. Hart.

We have no doubt that Dr. Marcet's volume will be extensively consulted, both in and out of the profession. We may add the suggestion, that a future edition would be rendered more complete by being provided with an index, without which no book should appear which is intended to serve for reference.

ART. VIII.—*Sull' Uso della Gomma-Resina d'Asa Fœtida adopterata per prevenire la Morte del Feto nelle Gravidanze morbose cagionate da inerzia dell' Utero.* Memoria del Dottore GAETANO LA FERLA, Vice-Presidente della Societa Medica di Malta.—*Malta*, 1855. pp. 29.

On the Use of the Gum-resin of Assafœtida in Preventing the Death of the Fœtus in Morbid Pregnancies caused by Inertia of the Uterus. By Dr. GAETANO LA FERLA, Vice-President of the Medical Society of Malta.

THE author takes credit to himself for discovering a power he believes to exist in assafœtida of preventing the death of the fœtus and abortion when

these phenomena depend, as he supposes, upon an asthenic condition of the uterus. Among the causes of this state he enumerates depressing mental emotions, terror, venereal affections, the abuse of mercurial inunctions, a relaxed constitution of the body, amenorrhœa, repeated menorrhagia, leucorrhœa, hysteria, and neglected miscarriage. His cases do not appear to us to be by any means conclusive; thus in the first, in which the death of the fœtus in three pregnancies depended upon syphilis, the venereal taint probably wore itself out; the fourth child—being the first born after the assafœtida treatment—living five days, while the children subsequently born continued to live (*rimasero in vita*). In the author's fifth case, his method succeeded after the patient and her husband had been subjected to anti-venereal treatment! His plan may be briefly stated to consist in the daily administration of assafœtida in such doses that from ten to fifteen drachms shall have been taken before the period at which the death of the fœtus occurred in the preceding pregnancy. All we should be inclined to say in favour of the efficacy of the system as reported, is, that it did not prevent the good results of the "strong" soups, the decoction of chamomile, and the goat's milk, which he wisely administered to the cachectic patients with whom he had to deal; and that it did not counteract his efforts to remove from them every source of annoyance and disquiet, to keep them agreeably occupied, and to inspire them with the hope of an auspicious delivery."

ART. IX.—*The Complete Handbook of Obstetric Surgery; or, Short Rules of Practice in every Emergency, from the Simplest to the most Formidable Operations connected with the Science of Obstetricy.* By CHARLES CLAY, M.D.—*London*, 1856. 16mo, pp. 290.

IN the preface to this work, the author informs us that he was originally led to prepare it for publication from the belief that no similar work existed in medical literature, and that one on such a plan was really required.

Without going quite so far as to admit that no such work exists in medical literature, we are quite willing to concede that we are not acquainted with one of a similar kind in the English language; and that, as far as we can judge, the author has succeeded in making it one of great practical utility. The work describes upwards of one hundred and eighty operations, many of which are not adverted to in the more ordinary treatises on midwifery. In several instances, the descriptions are illustrated by original and instructive woodcuts; and some of the articles, such as those on Embryotomy, Spontaneous Evolution or Expulsion, Hæmorrhage, Ovariotomy, and Version, are so elaborate and complete as to be worthy of taking rank with the best essays extant on these subjects.

The introductory chapter on Chloroform, although concisely written, contains some good general rules for its employment. We extract the following:—

"*First.*—IN CASES OF LABOUR. It may be used in severe, short, but ineffectual pains, which restrain bearing-down efforts. In these, chloroform renders uterine

contractions longer, stronger, and more efficacious; and thus it accelerates the accomplishment of the process.

"*Second.*—Where the parts are rigid and unyielding, it assists in dilating the parts, relaxes the muscular fibre, and relieves the severity of pain arising from rigidity.

"*Third.*—In long-protracted cases, worn down and suffering from nervous debility, and also irritability, it restores the physical powers, relieving both pain and anxiety.

"*Fourth.*—In some forms of convulsions it has been found useful.

"*It is not to be used.*—In convulsions of apoplectic or epileptic type:

"Or when the patient is strongly opposed to it.

"And even when the aversion to it is only moderate, it should not be urged.

"*The time for exhibiting it.*—It should not generally be given until the second stage of labour is established; unless some unusual severity of pains harasses the patient unnecessarily, when it may be used somewhat earlier." (p. 5.)

We agree also with the following:—

"*Necessary cautions.*—The pulse to be constantly felt, and if any untoward effects arise, the handkerchief to be removed.

"At first admit a free mixture of atmospheric air.

"Temperature of the apartment must be moderate.

"Patient not to be placed in deep insensibility (or snoring).

"Never commence chloroform in large doses.

"Preserve insensibility. Watch narrowly its effects.

"Never give chloroform immediately after a full meal, nor yet after long fasting. If a choice can be made as to time, select about two hours from the last food taken." (pp. 6–7.)

From the chapter on Embryotomy, we give the following quotation as illustrating the manner in which the subject is treated:—

"*Object.*—To reduce the child, to enable it to pass where the pelvic diameters are too small to allow a living child to pass from deformity or tumours; or where the head, from disease, is too large, though pelvis natural; and thus to save the mother at the expense of the child.

"*Necessary Conditions.*—When the head, though compressed, will not pass; when there is only just room for a mutilated fœtus to pass; when the forceps cannot effect delivery; when there is hydrocephalus.

"*Instruments.*—There are more in use than necessary; perforator, crochet, bone-forceps, craniotomy-forceps, cephalotribe, kephalepsalis, osteotomist. There is very seldom any necessity for more than the two first.

"*Dimensions of Pelvis requiring them.*—Antero-posterior diameter, according to Osborne, $2\frac{3}{4}$ inches; Clarke, $3\frac{1}{2}$; Burns, $3\frac{1}{4}$; Le Bois, 3; Aitkin, $2\frac{1}{2}$ to 3; Busch, 3.

"*Smallest Diameter allowing it.*—According to Dewees, 2 inches; Baudelocque, $1\frac{3}{8}$; Hull, $1\frac{3}{4}$; Burns, $1\frac{3}{4}$; Gardien, $1\frac{1}{2}$; Hamilton, $1\frac{1}{2}$; Davis, 1.

"*Mortality.*—To the child always; to the mother 1 death in 5." (p. 68.)

The subject of spontaneous evolution or expulsion is put very clearly before the reader, and illustrated by several diagrams copied from the work of Chailly. The explanation of the process given by Dr. Douglas in contradistinction to that offered by Dr. Denman, is, as we conceive, very properly insisted upon; the body of the child in these cases being doubled upon itself and expelled as in breach presentations, rather than rotated, so that the head emerges first from the pelvis—a mode of expulsion, it is almost unnecessary to add, which can only take place exceptionally, when the child is small and the pelvis capacious.

The chapter on Uterine Hæmorrhage is extremely comprehensive, and treats of the subject under the two heads of its general characters and varieties. The former comprising more particularly its statistics, dangers, symptoms, modes, and general treatment; and the latter its several varieties as occurring during pregnancy, during labour, and after delivery.

The next subject discussed by our author is Ovariotomy; the general history, symptoms, diagnosis, and treatment, remedial and operative, of ovarian tumours, are very fully but concisely set forward. Few men would appear to have had more experience in regard to extirpation of the ovaries than Dr. Clay, and few therefore have a right to be heard more authoritatively upon this question:—

"I have now operated," he observes, "seventy-one times (for extirpation of the ovary), and the general results are as follows—71 cases: 49 recoveries, 22 deaths. Taking these cases in groups as they occurred—

"The first 20 cases, 8 died, 12 recovered.
"The second 20 cases, 6 died, 14 recovered.
"The last 31 cases, 8 died, 22 recovered.

"Thus, the mortality has been gradually lessened.

"Of the first 20, deaths 1 in $2\frac{1}{2}$.
"Of the second 20, deaths 1 in $3\frac{1}{3}$.
"Of the last 31, deaths 1 in 4.

"Here, as it might naturally be expected, experience has gradually lessened the mortality in my own practice from 1 in $2\frac{1}{2}$ to 1 in 4. And I have a confident hope that it will be reduced still further, from improved diagnosis, experience in operating, and lastly, in the mode (which practice only can command) of after-treatment being better understood." (pp. 158–159.)

We fear, however, that the experience of others will not enforce the very favourable opinion thus expressed of the results to be expected from the operation. The facts collected by Dr. Robert Lee* are far from encouraging; and apart from the risks and dangers of the operation itself, which in many cases can neither be anticipated nor foreseen, we would submit that the difficulty of always making an accurate diagnosis must ever constitute an insuperable barrier to its frequent performance. Nevertheless, we admit that, under certain circumstances, it may be rendered justifiable; and in such cases the experience and practice of our author, as laid down in the present chapter, may be usefully appealed to.

ART. X.—*Researches on Pathological Anatomy and Clinical Surgery.* By JOSEPH SAMSON GAMGEE.—*London*, 1856. 8vo, pp. 216.

THESE memoirs, though exhibiting industry and powers of observation on the part of their author, would have found their proper place in one of the journals, whence, if thought desirable, they might have afterwards been collected, with the advantages of a more matured consideration and a less hurried composition than they now exhibit.

The papers are eight in number:—1. On Rupture of the Heart from External Violence. Mr. Gamgee relates an interesting case of this occurrence, and, from a survey of several recorded cases, comes to the conclusion that rupture of the heart, without penetrating wounds, takes

* Medico-Chirurgical Transactions, vol. xxxiv. p. 13.

place more frequently than is generally supposed. 2. On Dry Gangrene. Two cases are given. 3. On Cystic Sarcoma and Cancer of the Breast. The difficulties of diagnosis in tumours of the breast, when complicated with cysts, are dwelt upon; and the author suggests that some of the confusion that prevails might be obviated by substituting for the terms "cystic sarcoma," and "cystic carcinoma," the appellations "glandular, or benign tumour with cysts," "scirrhus, or encephaloid with cysts." 4. Treatment of Cancer by Landolfi's Paste. Mr. Gamgee is rather severe in his criticism upon the mode in which M. Landolfi's pretensions as a curer of cancer have been handled by the German medical press; but we should be glad to know how any man can be characterized other than as a charlatan who declares that "in 3000 out of 4000 cases which I have treated by this method, the patients had no recurrence of cancer," without furnishing any clinical proof of the accuracy of such a statement. A most adverse report was made by the Paris Commission. 5. On Syphilisation. The author here gives us an account of a visit he paid to Sperino of Turin, who seems to have thoroughly inoculated him with his doctrines. The most satisfactory part for us is to learn that the Professor has suspended his unjustifiable experiments at the *Syphilicome* upon the unfortunate beings committed to his charge. 6. On the Neapolitan Mode of performing Lithotomy. Mr. Gamgee describes the mode of performing the lateral operation as he witnessed it at Naples, and which he finds to be identical with that advocated by Moreau. The maximum mortality, he says, varies from 15 to 10 per cent. From a comparison of some of the results obtained from lithotomy and lithotrity, he gives a decided preference to the former operation; but the figures he adduces stand in need of a far more rigorous examination than he has bestowed upon them, before such conclusion can be accepted. 7. Treatment of Fractures of the Lower Extremity. The treatment by suspension is here described, and that by the starch-bandage recommended. 8. On Calcification and Ossification of the Testis. References are given to the various cases and specimens on record.

ART. XI.—*Index Morborum Internorum Systematicus. Præmissis de Nosogenesi Aphorismis.* Auctore O. BANG, M.D. et Professore in Universitate Hauniensi; Regi a consiliis Conferentiarum; Collegii regii cui summa rerum medicinalium in Dania cura incumbit, sodali; commendatore ord. Danebrogici et Wasa, cruce argentea ord. Danebrog. ornato; membro societ. med. Hauniensis, Dresdensis, et Hamburgensis honorario, Suecicæ, Norwegicæ, Bonnensis, Heidelbergicæ ordinario, Academiæ Imperialis Medicæ Gallorum correspondente.—*Hauniæ*, 1855. pp. 36.

A Systematic Classification of Internal Diseases; preceded by Aphoristic Remarks on the Origin of Disease. By O. BANG, M.D., Professor in the University of Copenhagen, &c.

So long as the candid physician is constrained to acknowledge that the intimate nature of disease, the actual derangements accompanying morbid conditions in the majority of instances, are incapable of positive demonstration, so long will the attempt to establish a sound and comprehensive system of nosology fail to meet all the difficulties that invest such

attempts. The foundation upon which any structure of the kind can as yet be built up, must necessarily be in a great measure hypothetical, and that hypothetical foundation as necessarily varies according to the individual mind that constructs it. This should not be the case with any department of inductive science. It is high time that the experience of the past should restrain men's minds from yielding to the fascination of dreamy speculations. Most of all does it behove men who occupy the responsible position of instructors, to beware how they hold up a Will-o'-the-Wisp to their auditors in the place of a steady and guiding light.

These remarks have been specially suggested to us by the perusal of Professor Bang's aphorisms, which form the basis upon which his nosological system rests. He commences by attributing disease to three different kinds of effects produced by the action of injurious influences (*potentiæ nocentes*) upon the human frame; according to his views, disease will manifest itself either in reaction to the influences, or by vicarious action in an organ for the one first exposed to injury, or by localization in an organ in which the influence may produce less harm than in the part first attacked. With all the respect due to a man in the position of the author, we cannot but think that, in arguing about vicarious action and localization as two distinct primary elements of disease, he establishes a distinction without a difference. But if we apply this mode of elucidating the nature of disease to any given case, how are we to determine into which category it fits? No indications are given by which we are to make our selection. Were this only a plaything of the learned Professor, which he takes up to beguile an hour, we should not be disposed to treat it seriously; but these three elements of disease are made the elements of his practice, for the treatment of disease is to be based upon them, and the aid they afford in its elimination. In paragraph 20, we read—

"Because in the chief symptoms of the majority of diseases we find nature exercising a curative influence, and the powers of nature are as it were the source of the diseases. The physician ought always to be the minister and interpreter of nature, and never disturb her efforts, so long as he thinks he may attain his end by his three remedies, reaction, vicariation, and localization." (p. 14.)

The classification itself recognises two main divisions: the first of these is the diseases of the cardinal systems, or universal diseases: these are subdivided into dynamic and material diseases; the former include angiopathia and neuropathia; the latter, hæmopathia, with all those affections ordinarily set down to the presence of a poison in the blood; eccrinopathia, or disorders of se- and excretion, and trophopathia, or diseases of nutrition. The second great division includes the diseases of organs or topical affections.

Art. XII.—*Memorie della Società Medico-Chirurgica di Bologna—Del Tetano Traumatico Trattato.* Del Dottore Leonzo Capparelli.—*Bologna*, 1855. 4to, pp. 210.

Memoirs of the Medico-Chirurgical Society of Bologna—A Treatise on Traumatic Tetanus. By Dr. L. Capparelli.

The Medico-Chirurgical Society of Bologna having, in January, 1853, announced a prize of 500 francs for the best monograph on Traumatic

Tetanus; but one, the treatise named at the head of this article, was sent in: and although it was found not to have fulfilled all the requirements of the proposed subject, it was, in consideration of its many and special merits, awarded the prize. In thus deciding, the Council of Adjudication acted, we think, wisely; the work before us is a full, elaborate, and interesting essay on a most important subject, the consideration of which is brought down completely to the present time.

The author divides his treatise into three parts, the first containing a description of traumatic tetanus, with the functional disturbances to which it gives rise; its diagnosis and its course. In the second part, he considers the causes, the pathological anatomy, and the pathogeny of the disease; and in the third, he discusses its treatment and prognosis. We can merely glance at a few points in one or two of the subdivisions of these heads.

The direct causes of traumatic tetanus may, as the name of the disease itself suggests, be classed under the single categorical head of lesion of the tissues, and they are studied by the author in three subdivisions: viz., violent lesions, artificial lesions, and sores. An examination of 125 well-established cases of the first of these varieties, shows that tetanus proceeded from punctured wounds, in 28 instances; from lacerated wounds, in 22; from gun-shot wounds, in 17; from contusions and contused wounds, in 15; from cutting wounds, in 11; from fractures, in 19; from burns, in 6; from dislocations, in 3; from bites, in 2; from pulling, in 2. With respect to the susceptibility of the several parts of the body, the following gradation was observed: the hand was the part injured, in 31 cases; the foot, in 30; the lower extremities, in 30; the upper extremities, in 14; the head and neck, in 11; the shoulder and back, in 6; the abdomen, in 3; the thorax, in none. The preponderance of punctured and lacerated wounds as causes of traumatic tetanus, and the greater liability of the disease to follow injuries of the extremities than those of the trunk, head, or neck, are well known; but reliable statistics are so important, that we quote the numbers given by the author. Artificial lesions are considered under the subdivisions of major and minor surgery (*alta e bassa chirurgia*); and of sores, not usually classed among the causes of tetanus, one produced by the application of caustic potash to a suppurated inguinal bubo, and a few other instances quoted from different authors, are brought forward as examples. From an examination of 116 cases of traumatic tetanus, furnished by his own experience and that of others, the author deduced the following results as to the ages at which the disease is most likely to occur: of these 116 cases, 41 took place at between 10 and 20 years; 37 between 20 and 30; 15 between 40 and 50; 14 between 30 and 40; 4 between 50 and 60; 4 between 1 and 10; and 1 between 60 and 70.

In reference to treatment, the author does not, we fear, add to our means of combating this formidable malady.

The seventh chapter of the third part treats of spontaneous recovery from traumatic tetanus; and the eighth and last, of the prognosis of the disease.

Art. XIII.—*Summary of New Publications.*

Among the books recently received, we would first advert to the 'Army Meteorological Register, for Twelve Years, of the United States,' compiled under the direction of Brevet-Brigadier-General Thomas Lawson, Surgeon-General to the U.S. Army, a work evincing great labour and research. We mention Dr. Conolly's important work on the 'Treatment of Insanity' merely to state that the review which we have prepared of it, unavoidably stands over to our next number. From India we have received a translation of a Treatise by Vogel, 'On the Disorders of the Blood,' carefully executed by Chundar Coomar Dey.

In Medicine we have to mention the appearance of a third and enlarged edition of Dr. Williams' admirable 'Principles of Medicine;' and a second edition of the very illustrative and practical Lectures by Dr. Todd, on 'Diseases of the Nervous System.' Dr. Bower Harrison has issued 'A Few Remarks on the Perforating Ulcer of the Stomach and Bowels;' and we have also before us a reprint of Dr. Laycock's paper 'On the Pathology and Treatment of Contagious Furunculoid.' A very useful exposition of the Varieties of Continued Fever, by Dr. Peacock, we warmly recommend to the student; and to the attention of the profession generally, Dr. Ballard's little book 'On Artificial Digestion.' From Germany we have received: the collected Essays of Professor Virchow, of which we hope to give an analysis in our next; a work by Lebert, 'Die Cholera in der Schweiz;' and the first two parts of a large work by Dr. Spiess, of Francfort, entitled, 'Pathologische Physiologie, Grundzüge der gesammten Krankheitslehre.'

Among recent Surgical Works we have to name a fourth edition of Professor Syme's 'Principles of Surgery,' and the completion of the second edition of Maclise's 'Surgical Anatomy.' An interesting Address by Mr. Langston Parker, 'On the Treatment of Cancerous Diseases by Caustics,' places this subject in a more hopeful light. Mr. Zachariah Laurence has issued a reprint of the papers 'On the Pathology of Cancer,' which have recently appeared in the 'Journal of the British Medical Association.'

In Midwifery we announce a new work by Dr. Rigby, 'The Constitutional Treatment of Female Diseases;' a useful little book by Dr. Swayne, entitled 'Obstetric Aphorisms for the use of Students commencing Midwifery Practice.' A new edition of the 'Dublin Practice of Midwifery,' by Dr. Maunsell, is before us; and Dr. Bedford's work on 'Obstetrics' has already, within little more than a year, attained the honour of a fourth edition. To Dr. Bozeman's 'Remarks on Vesico-vaginal Fistula,' we shall revert at a future time; while of Dr. Gardner's work 'On Sterility,' we would rather say nothing, but that it is our duty to protest against the breach of good taste manifested in some of the illustrations.

In Sanitary Science we have to add to the Essays of Drs. Barclay and Tripe,—to the subjects of which we hope to advert more fully in our next—the Parliamentary Blue-books 'On the Census of Ireland,' a work of great labour, in which Mr. W. R. Wilde, of Dublin, has borne an important part. They contain, among other valuable information, an

elaborate abstract of the Epidemiology of Ireland, which we shall take an early opportunity of introducing more fully to our readers. In conjunction with this Report, we may mention that the Registrar-General has issued his 'Seventeenth Annual Report of Births, Deaths, and Marriages.'

Dr. Royle, with the able assistance of Dr. Headland, has brought out a third edition of his well-known 'Manual of Materia Medica;' Dr. Wood, of Philadelphia, has forwarded to us an important work, in two volumes, on the same subjects; while a seventh edition of Dr. Dunglison's work, 'New Remedies, with Formulæ,' together with an eighth edition of the same author's 'Human Physiology,' have just come to hand: Third editions of two well-known English Manuals 'On Physiology,' by Dr. Carpenter and Dr. Kirkes, have to be mentioned; and a sixth edition of Fownes' 'Elementary Chemistry,' under the joint supervision of Dr. Bence Jones and Dr. Hofmann.

A Monograph, by Mr. Swan, 'On the Origin of the Visual Powers of the Optic Nerve,' deserves special mention; a valuable Monograph has also reached us from France, by M. Godard, 'On Monorchidism and Cryptorchidism.' 'The Mechanism of the Gubernaculum Testis,' forms the subject of an Edinburgh Prize Essay, by Dr. Cleland. Dr. Beale has published in a collected form his 'Researches on the Liver;' and the first two parts of a large work 'On Human Anatomy,' by Professor Henle, have come to hand, of which we will at present only say that they present a new feature, in form of illustrations by coloured woodcuts.

Mr. M'Cosh's 'Advice to Officers in India,' which may be recommended to young hands, has obtained the honour of a second edition; a work of an analogous character, 'Hygienic, Medical, and Surgical Hints for Young Officers of the Royal and of the Merchant Navy,' has been issued by Dr. Saunders, R.N.

The new volume of the Third Series of the 'Guy's Hospital Reports' contains numerous articles of great value, among which we would specially mention Dr. Taylor's article on Poisoning by Strychnia, since republished in a separate form. The thirty-ninth volume of the 'Medico-Chirurgical Transactions,' of which we hope to give an abstract in our next, is of the high character which has usually distinguished its predecessors; nor can less be said of the seventh volume of the Pathological Society. A series of excellent Addresses are on our table, which handle the Science and Polity of Medicine in a variety of aspects, and have been published 'by request,' the names of the respective authors are Drs. Parkes, Chambers, Richardson, Ramsbotham, Miller; and Messrs. M'Whinnie, De Morgan, and Inman.

We may not conclude this summary without bestowing a word of praise upon a little book by Mr. Rhind, dwelling upon the advantages to the invalid of travel on the Nile; and we would also introduce to our readers the new French bi-monthly periodical, with a name bearing a familiar sound, the 'Revue Étrangère Médico-Chirurgicale,' the first number of which appeared on the 16th October, 1856.

PART THIRD.

Original Communications.

Art. I.

An Essay on the Cause of the Coagulation of the Blood.*

By E. Brücke, M.D.

§ 1. The fact that blood, when removed from the human body, coagulates in a few minutes, is universally known. Three things happen as consequences of this removal—

1. The blood comes into a lower temperature.
2. Its motion is lost, and it is reduced to a state of rest.
3. It is brought into contact with air.

Formerly, therefore, coagulation was referred to these three causes, or to one or two of them.

On a little examination, it would hardly appear that a *decrease of temperature* would be sufficient to cause the phenomena of coagulation; for the blood of fishes, turtles, frogs, and other cold-blooded animals, is fluid at a still lower temperature; and yet if removed from the body, it coagulates, without undergoing any change in that particular. Hewson, the immortal inquirer into the properties of the circulating fluid, demonstrated that it is possible to *freeze* the blood while yet fluid, and that after being thawed again, it will coagulate in the ordinary way. He has pointed out that a high temperature hastens coagulation, and that a low one retards it. In common with many other physiologists, I have myself found this to be the case; but I cannot agree with what has been asserted by some, namely, that at a temperature near the freezing point, coagulation is entirely prevented; for I have seen blood coagulate at every temperature above 32° F., and even below that point, provided the blood itself was not frozen. Having caught the blood of a horse in a small, wide-mouthed glass vessel, I corked it, and placed it in a jar containing a mixture of chloride of calcium and ice, in which it was cooled, and kept thoroughly fluid during 1h. 30m. The whole was then taken into an ice-cellar, and buried in the snow. Twenty-four hours after, the freezing mixture had a temperature of 31° F.; nevertheless, the blood had coagulated on the surface and on the walls of the vessel, so that about six parts were fluid, and one part coagulated. Another portion of the same blood placed under similar circumstances, was found to be entirely coagulated on the

* This Essay was submitted to the physicians and surgeons of Guy's Hospital, on the 15th Dec., 1855, in competition for the Fifth Triennial Prize, founded by Sir Astley Cooper.

fourth day. In this case the temperature was 32° F. Similar effects resulted when the blood of frogs and of turtles was treated in the same manner. In some cases it had thoroughly coagulated in twenty-four hours, in others it was only covered with a thin film. Sometimes, however, I have found the blood of frogs remain fluid for eight days while kept in the snow, and that of turtles for four days; and yet this blood has afterwards coagulated when raised to a temperature of 54°—58° F. All these experiments were conducted in precisely the same manner, and the blood refrigerated as quickly as possible, so that I can only look to the quality of the blood for an explanation of the difference of the results.

The period of fluidity decreases slowly with the increasing temperature until it reaches about 50° F., when the coagulation takes place more rapidly. It is from this cause, I believe, that some writers have been led into the error of stating that blood, at a temperature lower than 50°, does not coagulate. Undoubtedly they did not watch long enough to observe the coagulation. Turner Thackrah, in the second edition of his 'Inquiry into the Nature and Properties of the Blood' (London, 1834), says, p. 67: "A temperature of 120°—130° considerably accelerates the concretion of the blood, and one of 100°—110° generally does so, but in a less marked manner. A temperature of 40°—50° retards coagulation." In these remarks he is fully correct.

§ 2. Hewson also showed that it is not possible to keep the blood fluid by agitation, even at the temperature of the living body. He knew that the blood, even in the living body, commonly coagulates in those parts in which it ceases to circulate; but he found that in the tied jugular vein of a dog, above two-thirds of the blood continued fluid, after having been at rest three hours and a quarter;* and in one case, a part of the blood in a dog's heart was found uncoagulated thirteen hours after death.† Therefore *rest alone* cannot be the cause of coagulation. The blood of a terrier, which was suffocated by fastening a hog's bladder over his mouth, I found entirely fluid seven hours and thirty-five minutes after death. Also the blood of a large spotted or Dalmatian dog, killed in the same manner, was quite fluid six hours and thirty-two minutes afterwards. Nevertheless, when removed from the vessels, coagulation in both cases took place in from seven to ten minutes. I have also preserved thoroughly fluid the blood in the hearts of turtles (Emys Europæa), while it has been at rest, six, seven, and even eight days, in a room having a temperature of between 30° and 34° F., three days in a temperature of 50°, and twenty-four hours in a temperature of 75°.

These experiments were conducted in the following manner. Having tied the great arteries half an inch from the heart, and also the vena cardio-pericardiaca,‡ when, after some time, the heart was swollen with venous blood, I tied the great veins also where they enter the auricles. The heart was then cut out, and suspended in oil previously heated with plaster of Paris, and washed with distilled water. Some time after the heart ceases to beat, the red particles subside, leaving the clear plasma

* An Experimental Inquiry into the Properties of the Blood. First Ed., p. 18. London. 1772.

† Ibid. p. 71.

‡ I give this name to the little vein represented in Bojanus, Anatome Testudinis Europææ. Vilnæ, 1819–1821, tab. xxix. fig. 160, æ 4.

above; but the fibrin does not coagulate until the last trace of life has disappeared. This experiment I repeated frequently with the same result; the blood always remained fluid in the heart, but when taken out it invariably coagulated, even when covered with oil.

§ 3. Hewson believed that atmospheric air was a powerful cause of coagulation. The chief experiment which induced this belief, he relates at page 20, as follows:—

> "Having laid bare the jugular vein of a living rabbit, I tied it up in three places, and then opened it between two of the ligatures, and emptied that part of its blood. I next blew warm air into the empty vein, and put another ligature upon it; and letting it rest till I thought the air had acquired the same degree of heat as the blood, I then removed the intermediate ligature and mixed the air with the blood. The air immediately made the blood florid where it was in contact with it, as could be seen through the coats of the vein. In a quarter of an hour I opened the vein, and found the blood entirely coagulated; and as the blood could not in this time have been completely congealed by rest alone, the air was probably the cause of its coagulation."

It is very true, and I have often proved it, that air blown in, in the manner he has described, hastens coagulation; but it is not always so. Having laid bare the right jugular vein of a shepherd's dog, which had been suffocated, I operated on it precisely as Hewson describes; and afterwards on the left jugular in the same manner, but without blowing in air. This experiment was performed at 10 o'clock a.m., and at 2·30 p.m. the blood in both jugulars was but incompletely coagulated, the fluid part coagulating after it had run out; and it was easy to perceive that the blood which was mixed with air had coagulated slowly, the red corpuscles having subsided so as to render the clot sizy. That this was not owing to a general want of coagulability in the blood of this animal, is proved by the fact that the blood in the other parts of the body coagulated not more slowly than in many other cases, so that by 4·45 p.m. the whole was solidified. The temperature of the room was 68° F.

I also tied the great arteries of an Emys Europæa, blew a quantity of air into the left subclavian vein, in order that it might mix with the blood in the heart, and then, having tied the veins, I cut the heart out, and laid it in oil purified as above mentioned. Twenty-four hours after, the blood was quite fluid, but coagulated when taken out. This experiment, repeated frequently and at a variety of temperatures, gave always the same result.

In June, 1854, I blew a quantity of air into the left subclavian vein of a living Emys Europæa; and the air having advanced into the arteries, I then tied the vein, covered it again with the sternum, and placed the animal in a cellar of 50° F. After it had been there five days, I found nearly the whole bulk of the plasma sanguinis in the great cisterna lymphatica; so that I could scoop it out with a watch-glass, in which it coagulated in from ten to twenty minutes. The bloodvessels had retained the corpuscles with very little plasma, and nowhere was there the slightest trace of a coagulum. This experiment, repeated three days after, gave the same result.

I also repeated it in a higher temperature. After having introduced the air, I brought the animal into a room of 75° F.; and after a lapse of

twenty-four hours, I let the blood out of the heart. It was foamy, but thoroughly fluid, and coagulated in the usual way.

I tied the two aortæ of an Emys Europæa, with a view to confine the blood entirely to the circulation of the lungs; and then established artificial respiration for the space of one hour. The blood became very fluid, but no clots were formed in the vessels; and when taken out, the process of coagulation was by no means uncommonly rapid. The first distinct film was formed in fifteen minutes; but even after a lapse of one hour and eight minutes, on cutting through the clot I found some drops of a coagulable fluid.

Turner Thackrah* cites cases in which the arterial blood coagulated sooner than the venous blood of the same individual; and it has often been disputed which of the two kinds usually coagulates the earliest. The truth is, that arterial blood may coagulate either slowly or quickly, and venous blood may do so too. There are *other* conditions which overpower the influence of gases so much, that that influence cannot be distinguished; but I observed that in numerous experiments in which the blood coagulated uncommonly slowly, it was extremely venous.

It is well known that bright blood, excluded from the air, becomes dark by changing its oxygen into carbonic acid; but it becomes even darker by its natural contact with the living heart and vessels. If, while a rabbit is being suffocated, a vein is suddenly opened, the blood flows out as dark as ink; and frogs' hearts, which, being distended with blood, are then tied and inserted into an inverted receiver full of mercury, become, under these circumstances, so dark, that every trace of red vanishes; whereas the blood placed in the same receiver apart from the heart, becomes only of a dark red. Blood which is thus deprived of its oxygen, coagulates more slowly than that drawn from the living animal; this I observed generally with respect to the blood of frogs, toads, and turtles retained in the heart, and thus kept in oil or in quicksilver. The slowest case of coagulation was shown by the blood in a turtle's heart which was kept for twenty-four hours in hydrogen gas. After having been exposed for an hour to atmospheric air, the first thin film was visible, which I removed; a new film was then slowly formed, which I also removed; a third film then appeared, but so slow was the operation, that the coagulation was not complete until about four hours after its commencement.

In warm-blooded animals, the blood abstracted an hour or two after death coagulates also slowly. Having suffocated a rabbit by drowning it, I took blood about an hour afterwards from the right jugular vein; and, in a temperature of 63° F., coagulation began in twenty-five minutes, and finished in forty minutes. Three hours afterwards I took some blood from the left jugular, in which there was already a slight coagulum; but the part which remained fluid concreted so slowly, that it did not begin until thirty minutes after; and even in the space of an hour it was not completed, the clot still containing some fluid drops, which, however, coagulated when removed and placed in a watch-glass; still, these striking cases are exceptional. I frequently took blood from suffocated dogs, several hours after death, which clotted in seven or ten minutes. The blood of

* Op. cit., p. 42. Lond. 1819.

living dogs coagulates in the air in from two to five minutes, and even sooner; but in warm-blooded animals there are two causes which retard the coagulation of blood taken after death—viz., first, the chemical alteration which the blood has undergone, and secondly, the reduction of its temperature.

It appears, then, that air assists coagulation, but not always powerfully. The contact of other foreign bodies with the blood in the vessels also hastens its coagulation in as great, or as it would appear, in an even greater, degree. Thus, having inserted a rolled platina wire into the jugular vein of a suffocated dog, I drew it out, after fifteen minutes, covered with a clot. Further, having inserted, by means of a glass tube, some mercury into the heart of a turtle, after having tied the great arteries, I then tied the veins, cut out the heart, and kept it for eight days in oil at a temperature of 36° F. On examination after that time, the mercury was found to have dispersed itself in numerous globules in the arterial trunks, and in the right auricle. The globules were covered with a clot, but the rest of the blood was fluid, and coagulated as usual afterwards. Virchow,* De Castelnau, and Ducrest also found coagula round the globules of mercury which they had conveyed into the right side of the heart of dogs and rabbits; and Magendie, Carswell, John Davy, and Schroeder Van der Kolk, each observed the coagulating power of other foreign bodies, when conveyed into the stream of the blood.

It has been often observed, that when blood is placed in a cup, coagulation generally begins on the surface, and that if it takes place slowly, a film can be removed from the surface, the blood below being fluid. But it must be remarked, that it takes place nearly as soon on the walls and on the bottom of the cup, leaving the middle of its bulk still fluid. Once, as has been already mentioned, some of the blood of a turtle remained fluid for four hours, exposed to the air in a watch-glass; and Hewson† relates some striking instances of the same kind: in fact, it is a general rule. The difference in the time between the coagulation of the outer and of the inner part of the blood, is, however, not always constant, and is of course the greater when the coagulation is slower.

§ 4. On the other hand, although many writers‡ derive coagulation from the influence of air; yet every precaution may be taken to exclude air, and nevertheless the blood, in its normal condition, cannot be kept fluid.

In the years 1853 and 1854, I instituted a series of experiments which enabled me to confirm the statements of those who assert that in general blood needs no air for its coagulation. By means of a glass tube united with a well-washed and dried caoutchouc tube, I took from the jugular vein of a living dog seven cubic inches of blood, and immediately passed it into an inverted graduated tube filled with mercury. This blood coagulated entirely, and without a buffy coat; which proves that it had not been so long fluid as to allow the red corpuscles to subside. This sign must be carefully looked for when the conditions of the experiment forbid

* Archiv für Path. Anatomie, von R. Virchow, u. B. Reinhardt, vol. i. p. 315.

† Op. cit., pp. 69, 70, 71.

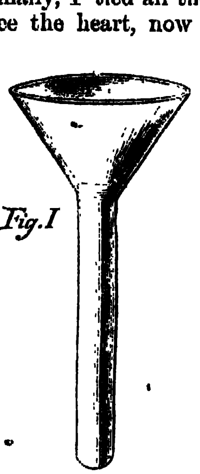
‡ The literature of the long-combated question, Whether or no air is the cause of the coagulation of the blood, is collected in Hamburger:—"Experimentorum circa sanguinis coagulationem, specimen primum." Diss. Inaugur. Berolini, 1839.

direct explanation. It must be remembered that the corpuscles of different specimens of blood subside in different times. This subsidence is greatly retarded by the motion which arises from the blood being warm while the surface is cooling by exposure to the air. This is one reason why the blood of the amphibia is more frequently buffed than that of mammalia and birds. Another reason is to be found in the larger size and smaller number of the corpuscles, as well as in the fact that coagulation does not so soon commence. Thus, the blood of some mammalia (for example, of horses) which coagulates comparatively slowly, has more tendency to produce a buffy coat than that of others. *Cæteris paribus*, the corpuscles subside sooner when fewer in number, and when they have a tendency to adhere to each other. Therefore, when blood whose corpuscles have once subsided, is again agitated, they fall more rapidly, because they are united into little clusters or rouleaux, which are not separated by the agitation.

In this experiment, the pressure upon the blood was 4·33 in. of quicksilver less than that of the atmosphere, but I did not discover the smallest air-bubble. Therefore I cannot agree with Sir Charles Scudamore, who thinks that the loss of carbonic acid is the cause of coagulation. In numerous other experiments in which blood was conducted into mercury without contact with air, it was completely coagulated without any loss of carbonic acid.

The blood of a turtle conveyed in the same manner into mercury, also coagulated without contact with air; but thinking it possible that the free oxygen circulating with the blood has some influence on coagulation, I instituted an experiment in which time was given it to combine with the organic substance before the coagulation could commence. I tied all the arteries coming from the heart of a turtle, and cut them below the ligature. I then tied the vena cardio-pericardiaca, and cut that also. Finally, I tied all the veins, and cut them above the ligature, in order to free the heart, now swelled with blood, from every connexion with the body. The heart, thus treated, was inserted below the glass vessel (fig. 1.), previously filled with mercury, and inverted over a quantity of the same metal. It was still regularly pulsating, and its contained blood became very dark. Twenty-four hours afterwards, finding the heart in a state of rest, I squeezed it by means of a curved crucible-forceps. The blood rose quite fluid in the mercury, but coagulated there without any contact with the atmosphere.

Fig. I

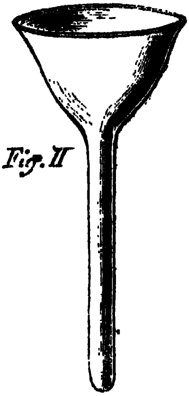


The glass vessel used in the experiments on the hearts of turtles (reduced one-half).

It is known that air adheres to the surface of glass, and exists there in a state of condensation. In order to discover whether this were a source of fallacy, I expelled this air by boiling the quicksilver in the glass vessel, and then repeated the experiment, but the result was the same; and in many trials, I never succeeded in keeping the blood of the turtle fluid in quicksilver.

§ 5. I was more successful in my experiments with the hearts of frogs. On May 27th, 1853, I tied the great arterial trunks of a frog; and when the heart was distended with blood, I also tied the veins, cut out the heart, and placed it under an inverted glass vessel (fig. 2) filled with quicksilver. It ceased to pulsate after one hour, and after five hours I squeezed it with the curved forceps, in order to liberate the blood, which then rose in the tube. Next morning the red corpuscles had subsided, a very small clot having been formed. The fluid residue was then exposed to the air, and coagulated thoroughly; and in half an hour had already thrown out some serum. The temperature was 68° F. I have repeated this experiment very often, and at different seasons of the year, but with different results. Sometimes the blood was not coagulated at all, even after having been for twenty-four hours free in the tube; sometimes it was in part coagulated, and sometimes completely so.

Fig. II



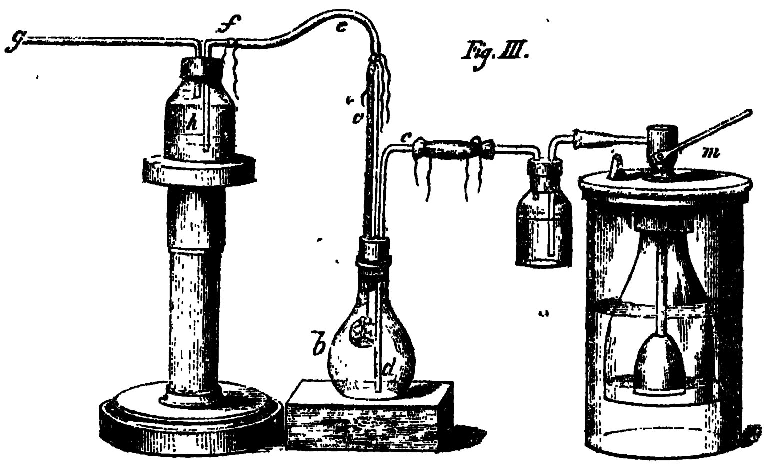
The glass vessel used in the experiments on the hearts of frogs (reduced one-half).

I succeeded in keeping the blood fluid more frequently in the cold season than in the warm; more frequently with frogs which had been kept for some time in captivity, than with those freshly procured; more frequently, if I liberated the blood after the heart had been twelve or twenty-four hours in the quicksilver, than if it had only remained there two or three hours. In some instances the blood did not coagulate, even when freed from the quicksilver, and exposed to the air; but this happened in the spring, when the frogs had been kept in captivity all the winter, and the blood was therefore deteriorated in a high degree, although it was not altogether deficient in coagulability, for that portion of it which flowed when the heart was cut out was found to have clotted.

I made similar experiments in a somewhat different manner, and with varying results. I placed the frogs' hearts, tied as before, in the same glass vessels filled with purified oil: and after six, twelve, or twenty-four hours, I squeezed them, so that the blood trickled down in the oil. In this condition I kept it even more frequently fluid than in the quicksilver; but I attempted in vain, under any circumstances, to retain in the fluid state the blood of recently-caught toads (Bufo cinereus) or of turtles. (Emys Europæa.)

§ 6. Further, I tied the great arteries and the vena cardio-pericardiaca of a turtle, and then the great veins, with a long thread of double silk, inserted the silk through a glass tube (fig. 3, *a.*), the upper opening of which was contracted, and having fixed the end of the silk to the outside of the tube with wax, I then cut the heart out. This glass tube (*a*) was already passed through a cork which fitted the mouth of the bottle (*b*), and the cork was also perforated by another tube (*c, d*), through which pure hydrogen was pressed into the bottle (*b*), in order gradually to expel the atmospheric air through the glass tube (*a*), the caoutchouc tube (*e*), the glass tube (*f*), the bottle (*h*), and the glass tube (*g*). I commenced

this experiment in the morning; in the evening, when all pulsation had ceased, I began rapidly to drive in the hydrogen, which was developed in an apparatus similar to Döbereiner's platina fire-machine (*m*). Then, untying the caoutchouc tube from the glass tube (*a*), I rapidly withdrew the



The apparatus described (one-fifth the natural size).

silk and replaced the caoutchouc tube. The heart fell into the bottle, and shed a quantity of dark and dichromatic* blood, which next morning, when I disunited the whole apparatus, I found to be coagulated, whilst the blood which still remained *in* the heart. was thoroughly fluid.

This blood being preserved in a watch-glass, was, after one hour only, covered with a thin film; and even in the afternoon, by cutting through the clot, I obtained some drops of fluid blood, which afterwards coagulated.

I varied the experiment by tying, not the veins, but the arteries, with the long thread of silk. This was done on the 1st of October, 1853, at 11 a.m. On the 2nd, at 8½ a.m., I found the heart without any movement. Having withdrawn the silk, as before, the heart fell, and dark dichromatic blood ran out, which in an atmosphere of hydrogen gas, and without any contact with air, completely coagulated in two hours. I repeated this experiment at different times, but I never succeeded in keeping the blood fluid.

I also repeated those experiments which Professor Brücke describes in the Monthly Reports of the Academy of Vienna, vol. x. p. 1070. He does not there speak at all about coagulation; but asserts that blood becomes dichromatic in hydrogen, nitrogen, and carbonic-acid gases; but not in oxygen, nor in common air. From experiments made on frogs, toads, turtles, and dogs, I am able to assert that dichromatic blood coagulates equally well with that which is not dichromatic.

The conclusions which I draw from all my experiments are as follows:

1. Air usually hastens the coagulation of the blood.

* Confr. E. Brücke; über den Dichroismus des Blutfarbestoffs; in Sitzungsberichte der Wiener Akademie, vol. x. p. 1070.

2. Air, when introduced into the heart and vessels of living turtles, does not induce coagulation.

3. The blood of frogs, when deteriorated by the action of the heart, or the other tissues of the animal, and so deprived of its free oxygen, sometimes requires atmospheric air for its coagulation.

4. Normal blood needs not the presence of air for its coagulation.

Therefore, and chiefly from the last conclusion, it follows that air is not that general cause of coagulation for which we are seeking.

§ 7. We have before seen that the blood in the living body is not kept fluid by animal heat, nor merely by its motion; and we are therefore compelled to admit that it is effected by other forces acting in the living body. These forces may be considered as of two distinct kinds:—one arising from the corpuscles of the blood, and the other from the walls of the vessels and the surrounding tissues. The first of these is easily excluded. Thus the lymph, which contains only a small number of cells, is fluid in the living body, and when withdrawn, coagulates like blood; and I have kept fluid during several days the clear plasma of the blood, in which the corpuscles had subsided, as in Sect. 2.

In another experiment made in July, 1854, I tied, in the manner previously described, the heart of a Testudo Græca; and having suspended it in purified oil, I placed it in a cellar, of which the temperature was 50° F. Three days after, I carefully opened the auricles at the highest part, and by means of a glass tube gently removed the clear plasma, and blew it into a watch-glass; and then removed the red fluid below in the same manner, and placed it in another watch-glass. The two portions thus separated coagulated in the same time. On repeating this experiment at different times, I found that the clear plasma usually coagulated more slowly than the part containing the red corpuscles. Therefore we must not look to the corpuscles as the source of the fluidity of the blood.

§ 8. Thus, by excluding the first-mentioned cause, we are led to the idea that the influence which keeps the blood fluid arises from the surrounding tissues—that is to say, from the heart, and the walls of the vessels; and I can adduce strong evidence that this idea is the right one.

It was Sir Astley Cooper who made the first successful experiments in support of it. In the first edition of Turner Thackrah's work (London, 1819), these experiments are related from memory, after a verbal communication from Sir Astley; and in the second edition (London, 1834), after a letter from the same, in which he says:

Exp. 1. Having carefully excluded the atmosphere from the ureter of an ox, I tied one end, and put a cock upon the other. The cock was tied in the jugular vein of a dog, and being then turned, the blood rushed into it. The cock was then shut, and the blood in ten minutes was found coagulated.

Exp. 2. The same experiment was repeated upon the jugular vein of the ox, which was, by the same means as the ureter had been, introduced into the jugular vein of the dog; and the blood coagulated in ten minutes.

Exp. 3. Two ligatures were placed on the jugular vein of a living dog, and there left for three hours. The blood had not coagulated.

Exp. 4. Two ligatures were put on the jugular vein of a living dog, leaving a space between them of three inches. Then the lower part of

the vein was cut through, and suffered to hang from the wound for four hours. The upper ligature was then removed, the blood admitted into the vein, and the ligature again tightened. The blood thus admitted into the dead vein was coagulated in a quarter of an hour.

Sir Astley Cooper induced Turner Thackrah to examine this subject, and the result was Thackrah's well-known work, in which he demonstrated by many experiments, that the blood in the excised veins of a recently-killed animal remains fluid at least half an hour, sometimes an hour, or even longer; while the blood of the same animal, received into the vein of another animal of the same species killed the day before or some hours previously, always coagulates in less than fifteen minutes. Therefore he agreed with Sir Astley Cooper, that "the vital or nervous influence is the source of the blood's fluidity; and its loss, the cause of coagulation."

His essay obtained a prize, and never was one more deserved. He found some followers, as Dr. Wright, Mr. Prater, and Mr. Ayres, but he never succeeded in making his views generally acknowledged. The chief reason, I think, was, that to most physiologists the difference in time did not appear sufficiently great to be striking. They objected, that a dead vessel allows diffusion rather than a living one; and that it was perhaps by the attraction of oxygen, or by the loss of carbonic acid, that the blood coagulated sooner in the former than in the latter; or, that possibly transfusion could not be performed with such care as completely to avoid the ingress of air, &c. Although these doubts were without foundation, they failed not of producing their effect on the minds of the medical public. It has also been objected that cold retards coagulation, and that blood may be frozen and become completely fluid again. If the theory of Cooper and Thackrah maintained that the *life of the blood* is the *only* source of its fluidity, then it might be asked, whether that life is able to bear a temperature of less than 32°; but, if the vital power of the *walls of the vessels* hinders the coagulation, by what reason is it *therefore* less conceivable that cold hinders, or at least retards it also? The coagulation of the blood has already been remarked by different authors, as the first step towards decomposition. The reasons are obvious. Coagulation is prevented by the influence of life, and retarded, although not absolutely prevented, by a low temperature. Blood which coagulates slowly, also decomposes slowly. Polli* bled a pneumonic man, thirty-seven years of age; the blood began to coagulate nine days after, and terminated within fifteen days; at the end of a month it began to putrefy. The temperature was between 8° and 11° Celsius (46°—52° F.).

But it cannot be denied that the want of complete success of Thackrah is attributable to his having confounded the action of the great nervous centres with the peculiar action of the walls of the vessels. He proclaimed that in inflammatory diseases the blood coagulates slowly, because vital action is increased; and that it coagulates rapidly when taken from a weakened frame, because the vital powers are reduced. He asserted, that in the case of an animal bled to death, the last blood which flows out

* Di un fatto relativo alla pretesa incoagulabilità del sangue in certe malattia.—Gaz. Med. di Milano, 1844, No. 3. Virchow, Gesammelte Abhandlungen zur Wissenschaftlichen Medicin part i. p. 114.

clots so rapidly, because the vital powers are vanishing. These explanations were, I think, erroneous; the difference not arising from the different state of the nervous system, but from the different quality and composition of the blood. Any one may convince himself that, if the animal, instead of being bled to death, is killed in any other way, the blood taken at the moment of death, or even one, two, three, or six hours after, coagulates not more quickly, but usually more slowly, than blood taken while life remained (§ 3). I cannot, therefore, support Turner Thackrah's views in general; but I can maintain, that the influence of the *living heart and vessels* is the source of the blood's fluidity, and its loss the cause of coagulation.

§ 9. It is an undisputed fact that the blood coagulates in the vessels of dead animals very slowly. In order to discover whether it is kept fluid by the lasting influence of the great nervous centre, I made the following experiment. On the 10th of May, 1854, at nine a.m., a terrier was suffocated by tying a hog's bladder over its mouth.* Forty minutes afterwards the brain was removed, care being taken to lose as little blood as possible, and the spinal cord was destroyed by means of a wire. At twenty minutes past five p.m., the blood in all, even in the largest veins, was quite fluid, and coagulated, when liberated, in the usual way. The temperature of the room during this experiment was between 62° and 65° F.

§ 10. I can adduce many instances in which the blood has kept fluid for a long time, without any connexion with the brain or spinal cord. I may refer to the experiments mentioned in Sect. 2, in which the blood was kept fluid for several days in the extirpated hearts of turtles and frogs preserved in oil. I also placed hearts, similarly treated, in glass vessels inverted over quicksilver, or in glass tubes, afterwards drawn out and hermetically sealed, while a stream of hydrogen was passing through. In all these cases the blood continued fluid. It is not even necessary to exclude atmospheric air. I suspended the heart of a turtle tied as before, and distended with blood, in a large bell-glass filled with atmospheric air, and inverted in water; and at the end of five hours the blood was quite fluid and coagulable. The temperature in this case was very variable, the room being heated for nine hours during the day, and cooling at night. Even the immediate contact of air does not induce coagulation of blood contained in the living heart; as I have shown in Sect. 3.

It may be asked, whether the fluidity of the blood depends upon the life of the heart, or not? Certainly if the heart undergoes decomposition, the contained blood is found to be changed into a viscous, uncoagulable fluid. I found the blood coagulated in the heart of a turtle which had been kept under oil twelve days in a constant temperature of 50°; but I frequently found the blood quite fluid, and in a state of perfect coagulability, in those hearts of turtles, frogs, and toads which were no longer affected by the most powerful electrical irritation. Notwithstanding, this fact cannot prove that a dead heart is capable of preventing the blood from

* It is to be observed, that in all my experiments wherein I inquired into the length of time during which the blood of dogs continues fluid after death, these animals were suffocated in the same manner; and that in most of my experiments the blood coagulated earlier than in this one.

coagulating; for we have seen that blood worn out by the frequent contractions of the heart, coagulates slowly, and may continue fluid a considerable time, even without the contact of the inside of the heart.

§ 11. In order to decide this question, I divided the great arteries of a turtle half-an-inch from the heart, and placed the animal in a cellar for three days. I then tied the arteries, and by means of a glass tube, transferred fresh blood from a living turtle into the heart so treated, then tied the veins, cut the heart out, and laid it in oil. Twenty-four hours afterwards the blood was completely coagulated, and had no buffy coat; which proves that it had coagulated at a very early period, for the blood of turtles usually becomes sizy during the first twenty minutes. I repeated this experiment several times, and on opening the heart one hour after, I always found the blood to be coagulated.

Thus the blood of turtles, as long as it is in a fresh and living heart, is always preserved fluid; but in a dead one it coagulates, as it would do in a glass or china vessel. But it must be carefully remarked whether the heart has really ceased to live; for often when it has remained quiet for a long time, if fresh blood is blown into it, it recommences its contractions. In one case I found this to happen eight days after death.

In another of these cases also I observed a very curious circumstance. I had bled a turtle to death, and kept it for three days in a room of from 65° to 71° F. At the end of that time, finding the heart quite empty and at rest, I tied the arteries, blew in the blood of a living turtle, and then tied the veins. The auricles of the heart began to act, and half an hour afterwards the blood had but partially clotted; but the fluid remainder when removed and placed in a watch-glass, coagulated almost suddenly, really in a few seconds, so rapidly as I had before seen only the last blood of warm-blooded animals—birds, rabbits, and dogs—when bled to death.

To this conclusion, that the power of keeping the blood fluid is dependent on the life of the heart, it may be objected, that the fresh blood which was blown into the dead heart had undergone the contact of air, and was thereby coagulated; but I have already shown, that in living hearts the entrance of air does not produce coagulation; and in order to avoid all sources of fallacy, I made the following experiments.

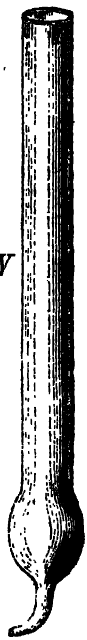
Having laid an open ligature round the great arteries of a turtle, I then cut the arteries below the ligature, and let the blood run into a cup. I then tightened the ligature, and by means of a glass tube blew some of the blood, through an opening in the left subclavian vein, into the heart again—tied it at the entrance of the great veins, then cut it out, and laid it in oil. It was kept in a temperature of 50°; and although some air-bubbles happened to enter with the blood, it was found quite fluid at the end of three days. The same result followed if I blew the blood of one Emys Europæa into the extirpated heart of another animal of the same species.

I then cut out the heart of a tortoise (Testudo Græca), and the blood being removed, I tied the arteries. I then cut the great arteries of an Emys Europæa, received the blood in a cup, and by means of a glass tube blew it into the heart of the Testudo Græca, and then tied the veins by tightening a ligature previously placed around them. This heart was kept in

oil at a temperature of 50°, and fifty hours after I found the blood quite fluid; but after being liberated, it coagulated thoroughly and firmly in forty-five minutes.

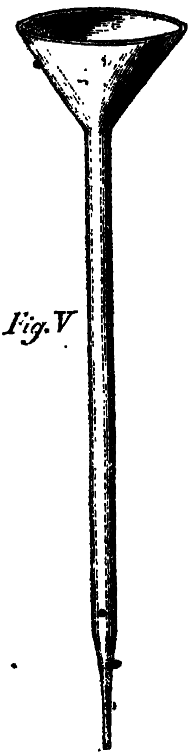
I exposed the blood of an Emys Europœa to the air for *fifteen minutes,* and kept it fluid during that time by means of cold, placing the open glass vessel which contained it in a mixture of water and snow. I then blew it back again into the heart, having previously tied the arteries. This operation was performed by means of the glass tube (Fig. IV.), into which it was poured through the funnel (Fig. V.). The veins being not tied, I cut out the heart, and suspended it, by means of wax and a thread, in a large receiver filled with atmospheric air, and inverted in water. Five and a half hours after I liberated the blood; it was quite fluid, and began to film over in ten minutes, and coagulated slowly, but firmly.

Fig IV



Glass tube for introducing blood into the living heart of a turtle (reduced one-half).

Fig. V



Glass funnel for pouring the blood into the tube, fig. 4, (reduced one-half).

§ 12. My next experiments were made with a view to determining whether the walls of the arteries and veins of the turtle possess the same power as the heart. For this purpose I tied the great arteries of a turtle at the distance of half an inch from the heart; and when they were distended with blood, I placed a second ligature on the bulbus arteriosus itself. These arteries being then cut out, were kept in oil in a room heated for nine hours daily from 59° to 63° F. Three days afterwards the blood was found quite fluid, and coagulated when taken out of the vessels. I placed similar arteries in the same room, in an atmosphere of hydrogen gas, and after three days the blood was quite fluid, and in a state of coagulability.

I hung the arteries and the left subclavian vein of an Emys Europæa, each distended with blood, under a receiver filled with atmospheric air, and inverted over water; temperature 59°. Twenty-four hours after, the blood was fluid, and coagulated on being liberated.

I also kept the blood of carps (Cyprinus Carpio) fluid, placing in oil the bulbus arteriosus, full of blood, and secured by a ligature; but it always coagulated more quickly than that of turtles. The longest time I suc-

ceeded in keeping it fluid at a temperature of 50°, was twenty-five hours; oftentimes it was completely coagulated in twenty-four.

§ 13. As might be anticipated, when the contact of the blood with the walls of the vessels is cut off by a foreign body, coagulation readily occurs. In order to demonstrate this, I placed round the great arteries of a turtle the three ligatures (*a*, *b*, *c*, Fig. 6).* I first tightened the ligature *b*, but in such a manner that it could easily be loosened; and then opened the pulmonary artery at *d*, and having inserted the little glass tube *e*, loosened the ligature *b*. After having allowed some blood to flow from the opening *d*, I tightened the ligature *c*, and finally *a* also. Afterwards the veins of the heart were tied, and the whole was cut out and laid in oil. Twenty-four hours afterwards, I found the blood in the glass tube firmly coagulated; but in the heart and in the other arterial trunks it was still fluid.

Fig. VI.

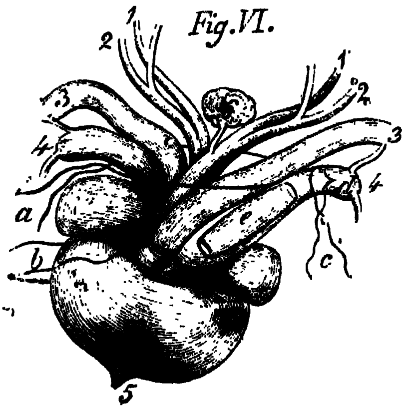
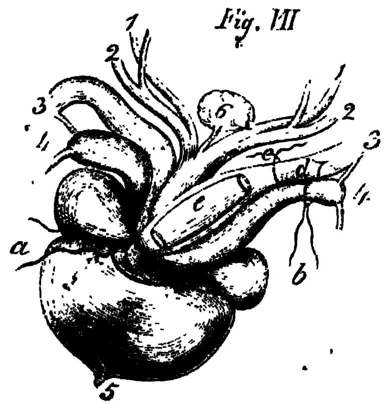


Fig. VII



Hearts of turtles, with the ligatures and glass tubes (natural size).

Again, I placed three open ligatures (*a*, *b*, *c*, Fig. VII.) round the bulbus arteriosus, the left pulmonary artery, and the arteria aorta sinistra, respectively. Having then tightened first *b*, and afterwards *a*, in such a manner that the ligature could be easily loosened again, I opened the aorta sinistra at *d*, introduced the glass tube *e*, unfastened the ligature *a*; and while the blood was pouring out at *d*, I tightened the ligature *c*. Fifteen minutes after, on fastening *a*, and opening the vessels, the blood in the glass tube was firmly coagulated, while that in the pulmonary artery was still fluid.

I think I have now plainly shown that the blood is kept fluid in the bodies of cold-blooded animals by the action of the walls of the heart and vessels; and that it coagulates out of the body, because it is then withdrawn from that influence. The difference of time in the results is here so striking, and the experiments are so much varied, that the objections

* Here, and in Fig. VII., the carotid is marked 1, the subclavian 2, the aorta 3, and the pulmonary artery 4; the vena cardio-pericardiaca 5, and the thyroid gland, with its artery, 6.

which were made to Thackrah's deductions cannot justly be applied to them.

Is it probable that the cause of the fluidity of the blood in mammalia and birds differs from that which obtains in amphibia?

§ 14. We have seen that the blood in the heart and vessels of a dog continues fluid for five, ten, and even thirteen hours after death; but that if removed, it coagulates generally in less than a quarter of an hour, and often in a few minutes. This difference cannot be ascribed to cold, for cold notoriously delays coagulation; nor to the influence of air, as was well known to Thackrah. He injected air into the jugular vein of a bitch. The blood, after death, flowed from the jugular vein, but coagulated on its effusion. Fifteen minutes afterwards, the blood in the vessels, though fully mixed with the injected air, remained fluid.

The experiment described in Sect. 3 may be also referred to, where blood mixed with air in the jugular vein of a dog, was but partially coagulated 4h. 30m. after; and on repeating the experiments of Professor Brücke,* I saw the blood of dogs coagulate in a few minutes in carbonic acid, nitrogen, and hydrogen gases, just the same as in oxygen gas, or in atmospheric air, although it entered immediately from the vessel into the glass tube filled with those gases.

Comparing these facts with the results of the fundamental experiments of Sir Astley Cooper and Turner Thackrah, it cannot be denied that the vessels of mammalia also possess the power of keeping the blood fluid; but it remains to be pointed out why this power in mammalia continues only a few hours after death, whereas in amphibia it lasts much longer. It is well known that the tissues of warm-blooded animals lose their irritability much sooner after death than those of amphibia; and I thought that possibly this might be the only reason, because I had found that in the class of birds which lose their irritability very early, the blood coagulates so quickly, that in the case of a cock which had been suffocated, the blood in the very heart itself, which had neither been removed from the body nor even laid bare, was coagulated one hour and a-half after death. I therefore attempted to keep the blood of mammalia fluid in the heart of a turtle, but in vain. For this purpose I opened the aorta sinistra of an Emys Europæa, and when it was as much as possible emptied of blood, I tied the wounded vessel, and by means of a glass tube, transfused into the left subclavian vein the blood of a rabbit. It passed through the arteries, but next morning it was found to have completely coagulated in the great veins and in the right auricle, notwithstanding that the irritability of the heart was not yet abolished.

Further, I received the blood of a horse in a glass cylinder, and placed it in a jar containing chloride of calcium and ice. Although I had to drive with it nearly a mile to my residence, the blood was quite fluid when it arrived there. I then filled with it four living hearts of turtles, two of which were kept suspended over water in an inverted glass jar, and in a temperature of 68°, and of the remaining two, one was kept in oil at 68°, and the other in oil at 50° F. Four hours afterwards, the three hearts kept at a temperature of 68° were opened, and the blood in them was found to be firmly coagulated. The heart kept in a tempera-

* Op. cit.

ture of 50° was opened twenty-four hours after, and the blood had also coagulated in it.

Thinking it possible that the blood had been too long withdrawn from the vessels, I determined to avoid this source of fallacy, and for that purpose kept a horse in a stall near my laboratory, and with his blood made four new experiments of the same nature, but so arranged that the blood, received in refrigerated glass cylinders, was, as soon as possible, injected into the hearts of the turtles. The first two of these experiments were made at a temperature of 71° or 72° F., and the hearts were opened seven hours afterwards. The remaining two were made at 70° or 71°, and the hearts opened six hours after; and in every case the blood was coagulated.

If, in these experiments, the blood of horses had remained fluid as long as the blood of turtles had done in the experiments mentioned in Sect. 11, they would permit the conclusion, that the blood of mammalia coagulates earlier after death than that of amphibia, only by reason of its higher temperature; and because the vitality of the heart and walls of the vessels disappears earlier after death in the former than in the latter; but as it is, they lead to no certain conclusion.

§ 15. A temperature approaching to that of the warm-blooded animals diminishes in a striking manner the time during which the blood of amphibia can be kept fluid. I have made some experiments with the hearts of turtles distended with blood, and placed in a hatching-oven. In the first case, the heart was kept in the oven during eight hours and three-quarters, the temperature meanwhile slowly decreasing from 95° to 89° F. At the end of that time the blood was fluid and coagulable. The second was kept for twenty-three hours in the oven, the temperature decreasing from 95° to 84°. In this case the blood was coagulated, but not firmly; nevertheless, the fluid which escaped when the clot was divided did not coagulate when it was exposed to the air. In the third experiment, the heart remained twenty-three hours exposed to a temperature decreasing from 95° to 89°. The result corresponded with that of the last. In the fourth case, the heart was kept in the oven for 12h. 40m. at a temperature decreasing from 97° to 91½°. At the conclusion of the experiment, some small flaky clots showed that coagulation had commenced, and the remainder of the blood clotted firmly when exposed to air at the ordinary temperature.

On a consideration of these facts, it must be acknowledged that, as far as regards the question before us, the difference between warm and cold-blooded animals is one of degree only, and not of kind. The blood of a dog just killed, which is reduced slowly from a temperature of 102°, remaining fluid, on the average, seven hours in the heart and vessels; and the blood of the turtle continuing fluid in the heart for about twelve hours, if kept in a hatching-oven, whose temperature gradually diminishes from 97 to 91½° F. Both are equally kept fluid by the action of the heart and vessels.

But it has not yet been demonstrated whether this difference arises solely from the unequal temperature, and unequal duration of vitality; or whether the blood of mammalia and birds has, by its own nature and composition, a stronger tendency to coagulate, and therefore requires a

more powerful action of the heart and of the walls of the vessels in order to keep it fluid.

§ 16. This question was settled by other experiments. On Nov. 11th, I tied the heart of a hedgehog (Erinaceus Europæus), and the right ventricle being swelled with blood I cut it out, and suspended it in a large bell-glass filled with atmospheric air, and inverted in water. The blood of the hedgehog lost during the operation coagulated in less than five minutes; but this animal possessing great tenacity of life, and the irritability of its muscles continuing longer than in other mammalia, I might hope to keep it fluid for a considerable time in the separated but living heart. This expectation was verified; in three hours and a half I saw the last faint contraction, and then, having waited another hour, I opened the heart. Coagulation had already begun;—in the pulmonary artery was a soft clot, commencing at the point where it was tied, and extending down into the ventricle; but beyond that, about two-thirds of the whole bulk of the blood was completely fluid, and coagulated in a watch-glass in ten minutes. By means of Neef's magneto-electromotor I could yet excite contraction in the right auricle, and a faint and scarcely perceptible movement even in the right ventricle.

Thus have I preserved fluid the blood of a warm-blooded animal in the extirpated heart for four hours and a-half in a temperature decreasing from 68° to 64°; but coagulation commenced as life began to vanish; whereas, the blood of frogs, toads, and turtles, preserved in their own hearts, always remains fluid longer than contractions can be excited in them.

The blood of a kitten showed less tendency to coagulate. I performed a similar experiment with a kitten's heart, and opened it after three hours and a-half. In the pulmonary artery only was there a clot; the blood in the right auricle and right ventricle was fluid, and the corpuscles had so subsided, that they were all collected in the ventricle, and the auricle was filled with clear plasma. The blood coagulated in a watch-glass in ten minutes; but the heart could no longer be excited to action by the magneto-electromotor. The temperature was 66° F. In another experiment of the same kind, and conducted at the same temperature, the blood of a kitten was preserved fluid for four hours; but the blood of a puppy treated in the same manner, was found coagulated after four hours and a-half, although the heart had not yet entirely lost its irritability.

Therefore the different permanence of vitality in the tissues and organs of cold and warm-blooded animals, although the chief reason of the difference above mentioned, is not the only one; the blood of warm-blooded animals having generally, though not always, more tendency to coagulate, and therefore requiring a more powerful energy of life to counteract that tendency.

Here also we meet with an explanation of the results obtained by Sir Charles Scudamore, who, in his fiftieth and fifty-first experiments, found the blood of a sheep to coagulate very rapidly (in four and five and a-half minutes), if received in the fresh jugular vein of another sheep. But it cannot be doubted that the vein, being cut out or laid bare, had already lost too much of its vitality to keep the blood fluid. It must also be recollected that the blood of sheep generally coagulates very quickly, much more quickly, indeed, than that of horses, dogs, or oxen. Sir Charles himself

does not deny the influence of life. He found blood intercepted by means of ligatures in the jugular vein of a horse, remain fluid an hour, and an hour and three-quarters, and yet coagulating, if let out, in five minutes.

§ 17. The blood is kept fluid by the walls of the vessels—the blood is also kept fluid by the heart—and lymph is fluid in the lymphatic vessels; it is therefore possible that blood can be kept fluid in the lymphatics. Warm-blooded animals were not fitted for experiment on that point, and I therefore again had recourse to turtles. I inserted a Cooper's forfex (having previously passed round its path an open ligature) between the lung and the stomach into the cisterna lymphatica, and divided the aorta sinistra near its anastomosis with the aorta dextra. I then withdrew the forfex, and closed the ligature. The great cisterna lymphatica being thus filled with blood, I tied the great veins and arteries, cut out the heart, and placed the animal in a temperature of 69° F. Seven hours and a half afterwards the blood in the cisterna lymphatica was quite fluid, and coagulated quickly and firmly when let out. This experiment, repeated at different times, and in different temperatures, gave always the same result. In order to determine whether the blood would likewise continue fluid in serous cavities, I passed a cutting cataract needle (also with an open ligature round its path), downwards from the shoulder through the cellular tissue in an oblique direction, into the pericardium of a turtle, and wounded the heart so that the pericardial sac was filled with blood; I then withdrew the needle, and tightened the ligature. One hour afterwards, I always found the blood in the pericardium to be firmly coagulated.

§ 18. It is frequently observed that the liquor pericardii contains liquid fibrin which coagulates when exposed to air. Therefore, although the pericardium does not keep normal blood in a fluid state, yet has it kept fluid fibrin when dissolved in liquids of a different composition. It cannot, however, be said that it does so by means of a peculiar action.

We have seen that in some cases (Section 5) abnormal blood did not coagulate until it had been some time exposed to the air. Therefore it is possible that the fibrin in the liquor pericardii may have continued fluid only because it was not exposed to the air. Virchow* has collected a number of cases in which fluid exudations were removed from the body long after death, and coagulated when exposed to the air; whilst, in a still greater number of cases, fibrinous exudations into the cavity of the pleura, or the pericardial sac, coagulate even in the living body. He never found coagula in the lymphatics of dead bodies, if they were in a normal condition; and he therefore thinks that the normal lymph in man never coagulates until it has been exposed to the air.

John Hunter relates a very remarkable instance in which blood, in a case of hydrocele, was kept fluid for sixty days in the tunica vaginalis; but there was in this case not blood alone, but blood mixed with the hydropic fluid.

In the tubuli uriniferi of the kidney, effused fibrin generally soon coagulates; but occasionally it does not do so, but is excreted dissolved in the urine, and afterwards coagulates if exposed to the air. I have myself seen a very interesting case of this kind. But there is no doubt

* Gesammelte Abhanlungen zur Wissenschaftlichen Medicin, p. 104.

that the composition of the fluid has more influence on the result, than the nature and action of the walls of the bladder, the ureter, and the tubuli uriniferi.

In the intestine of a recently-killed animal, blood has also been kept for some time fluid; but this experiment has not always produced the same result,* and leads to no certain conclusion, because the alkaline mucus which lines the internal surface of the intestine, may perhaps exert some influence.

§ 19. As it may happen that blood may be so altered in its chemical constitution as to continue fluid under circumstances in which normal blood would coagulate (Section 5); so it is also possible that blood may undergo an opposite change, and coagulate where normal blood would have remained fluid. No one, however, has given any explanation of the nature of that change, and I myself met with no opportunity of inquiring into it.

The hypothesis that blood rich in fibrin has a tendency to coagulate in the very vessels, is an ill founded one. No one has demonstrated a case in which the blood has coagulated during life by reason of the superabundance of fibrin. The white or yellow clots found in the hearts of those who have died of pleuritis or pneumonia, are notoriously formed long after death; and are indeed nothing but the buff of the blood which has coagulated in the heart, in the dead body;† they therefore rather prove that the blood coagulated in the heart unusually late after death, than that it did so during life. It is also matter of frequent observation, that blood drawn in cases of pneumonia or pleurisy, generally coagulates slowly, but is richer in fibrin than healthy blood. On the other hand, if an animal be bled to death, the last blood which trickles from it coagulates almost suddenly, and even that portion which yet remains in the vessels, clots with great rapidity. Such blood contains very little fibrin. I bled a dog to death, catching the blood in five different cups successively, and then carefully measured the quantity of fibrin which each cup contained. The result was as follows:—

	Blood, in grains.	Fibrin, in grains.	Fibrin, per cent.
I.	1588·5	3·55	0·223
II.	2146·7	4·28	0·200
III.	2381·9	4·22	0·177
IV.	2935·8	4·74	0·162
V.	1864·7	1·25	0·068

In a second experiment, also made upon a dog, but in which the blood was received in four cups, the result was as follows:—

	Blood, in grains.	Fibrin, in grains.	Fibrin, per cent.
I.	1670·7	4·86	0·291
II.	2086·5	5·64	0·270
III.	2491·6	6·07	0·244
IV.	930·5	1·71	0·184

* Sir Charles Scudamore relates, in his forty-eighth experiment, that the blood of man coagulated in the intestine of a recently-killed rabbit more quickly than it did in the intestine of a rabbit which had been killed the day before; and this result he rightly derives from the higher temperature of the freshly killed animal.

† Hewson. Op. cit. p. 44.

The blood of horses, which is by far richer in fibrin than that of dogs, coagulates much more slowly. All these facts are so striking, that some physiologists have thought that there is a direct relation between the quantity of fibrin in the blood, and the time which it requires to coagulate; but Nasse* found that it was not so. There is no doubt that the time required for coagulation is dependent upon so many different circumstances, that it is very difficult to determine the exact amount of influence exercised by any one of them.

Again, it is not proved that a constitution which causes the blood to coagulate rapidly when withdrawn from the body, produces likewise a tendency of the same blood to coagulate in the living vessels. Let us therefore only consider those cases in which blood coagulates in consequence of anomalous external conditions into which the very blood-vessels themselves of the living body may happen to fall. If we tie an artery, a clot forms from the ligature as high as the next branch springing from the same vessel. That blood, therefore, which was brought to a state of rest, has coagulated. Was rest the sole cause of this coagulation? We can imagine that although in amphibia, blood can be kept fluid for a long time in a state of rest, yet that in the blood of mammalia it may be necessary that fresh particles shall be constantly brought into contact with the walls of the living vessels, in order to keep it fluid. But we must first carefully inquire whether there may not be some other circumstances which exercise an influence in the case of man. Undoubtedly, the walls of the vessels are injured by the act of tying the ligature, and the internal coat is usually burst at the spot where the ligature is applied; and it is generally admitted that at *that* spot the coagulation begins, and increases upwards. M. Notta observed a very interesting case of a man, one of whose crural arteries was tied, and who died twenty-nine hours after the operation. On examination of this case, a branch was found springing from the artery above the ligature, so small as scarcely to admit an Anel's probe. This branch had not entirely prevented coagulation, being so small; nevertheless, the blood had not been quite at rest, a small quantity still continuing to run through the little branch. A small clot was found arising from the spot where the ligature was applied, and having a long filamentous appendix extending upwards to the next collateral branch, which sprang six centimètres above the first. Such cases show that the injury done to the walls of the vessels by a ligature, is not without its effect on coagulation. Every surgeon is aware that, in the ordinary cases, in which the clot fills the vessel as high as the next branch above the ligature, the walls of the vessel enclosing the clot undergo a change; the artery in this place becoming transformed into a solid string. Can the commencement of that change have any influence in bringing the blood to a state of coagulation? The formation of the clot is usually completed in thirty-six or forty-eight hours;† and in one case observed by M. Notta, in eighteen hours. Certainly in that period the change in the walls of the

* Rudolph Wagner's Handwörterbuch der Physiologie, tom. i. p. 105.

† It sometimes requires a longer time in tied *veins*. Having made a very small wound, I carefully tied the right jugular vein of four rabbits. On examining them forty-eight hours after, a clot was only found in two of them.

vessels cannot be far advanced; but yet it cannot be denied that their normal conditions are altered from the very moment that the blood within came to a state of rest. Therefore it is not impossible that the coagulation of the blood and the alteration in the nutrition of the walls of the vessels being contemporaneous processes, may promote each other by mutual action.

In general, if an arterial trunk be anywhere compressed by a tumour, or obstructed by a coagulum conveyed into it from the veins,* or by any foreign body whatsoever, the whole bulk of the blood reduced to rest, coagulates; but we do not exactly know how much time it requires; and we cannot with certainty decide whether rest alone, that is, the want of renewed contact with the walls of the vessels, is the cause; or whether the walls of the vessels are altered by the continued contact of the same portion of blood, and therefore allow it to coagulate.

In sound vessels, circulation may be greatly impeded, although not abolished, and notwithstanding, the blood remains fluid. For instance, long since, John Hunter observed that even in the most intense inflammation, blood does not coagulate, unless it terminates in gangrene.

On the other hand, blood will coagulate where the circulation is but slightly impeded, or somewhat slower than in the normal state; but in these cases the walls of the vessels have degenerated, or locally mortified. Then one thin layer of fibrin is deposited after another, until the vessel is obstructed.

But even when there is no impediment at all to the circulation, local disease of the walls of the vessels may produce depositions of fibrin; that part of the blood which touches the wall of the vessel being always more slowly propelled than the rest. It is extremely interesting to read Professor Virchow's descriptions of the various fibrinous depositions in the vessels, not only because he has collected a valuable and extensive series of cases, but also because these cases exhibit in a striking manner the influence which the walls of the vessels exert on the fluidity of the blood.

Any one who has read Professor Virchow's different papers on fibrin,† is aware how far he is from agreeing with the views of Sir Astley Cooper and Turner Thackrah, which I have supported; yet notwithstanding, he is compelled, by the irresistible power of facts, to appeal to the influence of the walls of the heart and vessels. In his paper on Arteritis,‡ he says:—"If the smooth surface of a globule of mercury is sufficient to make the blood coagulate round it; a spot in the inner coat of the vessel, when it is altered in its molecular condition, although still remaining smooth, must also be able to do so."

§ 20. It will necessarily be inquired, what idea I have formed of the nature of that peculiar action of the heart and vessels to which I ascribe the fluidity of the blood? Blood continues fluid in the sound and living vessels, but it coagulates in dead ones, and in jars or cups of every known substance,—glass, china, platina, silver, copper, &c., even without the

* Confer:—Virchow, Thrombose und Embolie, in his Gesammelte Abhandlungen zur Wissenschaftlichen Medicin. Erste Hälfte, 219.

† Archiv für Pathologische Anatomie, herausgegeben von R. Virchow und B. Reinhardt, vol. i. p. 321.

‡ Gesammelte Abhandlungen. Erste Hälfte, pp. 57 *et seq.*

contact of air. The inference drawn from these facts, is, that the vessels of the living body preserve the blood in the fluid state by means of a peculiar action which vanishes with their life. Would it not perhaps be better to say, that the contact of all bodies induces coagulation of the blood, except only the inside of the living vessels, which is so indifferent to the blood as not to do so?

This is a question which I have not been able to discuss at an earlier stage of the inquiry, because it necessitates a knowledge of many facts propounded in the foregoing pages. Undoubtedly the contact of foreign bodies promotes coagulation. In every cup or jar, coagulation begins from the surface, from the sides, and from the bottom, and proceeds towards the centre; and foreign bodies produce clots, even in the living vessels. But in blood withdrawn from the living vessels, coagulation proceeds from the foreign body throughout the whole mass; whereas a foreign body introduced into the living vessels, produces only a local clot, the remainder of the blood continuing fluid. Nobody doubts that the contact of air promotes coagulation; and notwithstanding, we have seen considerable quantities of air in the living heart and vessels, without any trace of coagulation. The blood of mammalia coagulates even in the living vessels, if brought to a state of rest; but motion *per se* does not preserve the blood fluid, nor does rest make it coagulate. Motion imparted to blood withdrawn from the vessels, hastens coagulation; and the blood of amphibia in the extirpated heart, and in a state of rest, remains fluid as long as that heart shows the slightest trace of irritability, and even longer. Yet we have seen that the blood of a rabbit requires sometimes more than forty-eight hours of rest to coagulate in a living vessel. If motion keeps the blood fluid in the living vessels only, it must be by means of the constantly-renewed contact with the walls of those vessels; and must therefore arise from some peculiar virtue in those walls. If the blood, being at rest in a living vessel, coagulates, it must be either because the blood requires a constantly-renewed contact with the walls of the vessels, or because the vessels require a constantly-renewed contact with the blood, and lose their normal quality if they are a long time in contact with the same portion of blood.

From these facts, I think we are compelled to assume that the walls of the vessels are not indifferent to the blood, but that they counteract its tendency to coagulate by a peculiar virtue inherent in them. In what this virtue consists, I am unable to say; and I wished not to waste time in planless experiments directed to that end. I thought it necessary that we should first understand the process which is impeded, and afterwards inquire into the nature of the power which impedes it. Therefore the next question which I proposed to myself was,—what changes does the blood undergo during its coagulation? Long did I labour in vain, being misled by generally-received, but erroneous opinions and prejudices; and I learned by experience how much truth there is in the remarks contained in the preface to the work of Turner Thackrah, who there says:—"The erroneous notions and unfounded theories which have been vainly adduced to remove the veil of Nature, have greatly obstructed the path of inquiry, and added darkness to obscurity." But latterly I believe I adopted a better mode of procedure, and I will briefly explain the results I obtained.

§ 21. If a solid body is produced from a liquid, then one of two things must happen: either it takes place in consequence of a change in the atomic constitution of the liquid, or it takes place without such a change. In the latter case, either the liquid itself undergoes a change in its cohesion—as, for example, when water freezes into ice; or a substance dissolved in a fluid becomes again solid, either by a change of temperature, or by a diminution of the dissolving fluid. If blood coagulates, it is a case of a solid body arising from a liquid, at the expense of substances previously dissolved; but it happens neither from a change of temperature, nor from a diminution of the dissolving fluid. But we know that a fluid can be so overloaded with a dissolved substance, that the molecules, even without any change of temperature or diminution of the dissolving fluid, suddenly rush from unstable into stable equilibrium; as happens, for example, if, into a super-saturated solution of sulphate of soda, is thrown a crystal of the same salt. It might be imagined that the blood could be such a super-saturated solution, and therefore coagulate when removed from its ordinary conditions; but there are various reasons which militate against this idea. We will here cite two of these, as sufficient to prove that it is inadmissible. First, if the blood has once thoroughly coagulated, and the clot contracted, no more fibrin can be obtained, either by increasing or diminishing the temperature, or by evaporation. Secondly, blood which contains a greater than ordinary amount of fibrin, does not therefore coagulate more easily; even the blood of a pleuritic patient forms its dense and firm clot slowly; and the last blood which flows from an animal bled to death, coagulates very quickly, although it produces very little fibrin. (Compare Section 19.) Therefore it must be admitted that blood coagulates in consequence of a change in its atomic constitution; but since we know that it does so without either losing or acquiring anything, it must therefore occur by a mere change in the arrangement of its atoms.*

§ 22. We may be sure that the material for the formation of the solid fibrin has the general qualities of all albuminized substances; but we can say no more,—we cannot even assert that it has more resemblance to coagulated fibrin than it has to albumen or to casein.

But although the coagulation of blood arises from the deposition of an albuminoid substance (or protein compound), this is not the only deposit formed during coagulation. I digested well-washed fibrin, procured by whipping the blood of oxen, in cold water containing $1\frac{1}{2}$ part of hydrochloric acid in 1000. Having evaporated the fluid in lukewarm water to the sixth part of its bulk, I added nitric acid, which precipitated a whitish substance; this precipitate being washed and filtered, evaporated with nitric acid, and mixed with ammonia, gave the yellow colour of xanthoproteate of ammonia.

* It must be observed that blood, during its coagulation, does not alter its reaction with litmus. I have made some experiments on the reaction of the plasma of the blood of horses and turtles. I extracted the common litmus with hot water, and precipitated the concentrated and filtered fluid with alcohol—then filtered it again, washed it with alcohol, and finally dried it. The blue litmus thus obtained was freshly dissolved for every experiment, in order to prepare a blue and a violet tincture, which were mixed with the plasma. The plasma of horses was always alkaline: that of turtles, either slightly alkaline or precisely neutral; in one single case it changed the blue tincture to violet. In this case the blood had been a long time in a tied heart, but coagulated as usual when liberated. The lymph of all turtles had a decided alkaline reaction.

After having precipitated with nitric acid, I filtered and neutralized the solution with ammonia, which slowly produced a deposit. In this deposit I discovered by means of the microscope crystals of the double phosphate of ammonia and magnesia, and a greater quantity of amorphous granules. To show that this granular matter was phosphate of lime, I filtered the precipitate, and dissolved it in water containing some acetic acid; and then added some drops of a concentrated solution of oxalic acid. I thus obtained a crystalline precipitate of oxalate of lime. The fluid separated from the phosphatic deposit by means of filtration, when mixed with phosphate of soda or phosphate of ammonia, gave a still larger precipitate of the double phosphate of ammonia and magnesia, and phosphate of lime. Therefore the original fluid had contained magnesia, lime, hydrochloric and phosphoric acids; but the latter was in far too small proportion to form phosphate of lime and the double phosphate of ammonia and magnesia, with the whole quantity of magnesia and of lime that was present. No sulphuric acid was detected.

I made the same experiment with the fibrin of human blood taken from the hearts of dead bodies, carefully washed with water, and digested with alcohol and ether; and the result was the same. I then placed the fibrin of the blood of oxen in acetic acid which contained thirty-five per cent. of pure acid, and laid it aside for some weeks in a room whose temperature varied between 32° and 41° F. The liquid was then concentrated by evaporation, and treated as before; and the effect was the same, only the precipitate, which fell down on the addition of nitric acid, was greater in quantity. In the former experiments I found that if I exactly neutralized the hydrochloric solution with ammonia, the albuminous matter fell down with the phosphates, and nitric acid gave no further precipitate in the filtered fluid; but if I exactly neutralized the acetic solution in this experiment, only a part of the albuminous substance fell down, and the filtered solution still gave with nitric acid a white precipitate, which, when evaporated with the acid and treated with ammonia, gave the yellow colour of xanthoproteate of ammonia.

In order to get rid of the albuminous matter without the intervention of nitric acid, I put the fibrin which I used in the last experiment, still swelled by the acetic acid, into spirits of wine of 0·280 sp. gr., leaving it therein for some days, and frequently agitating it. The spirit was then poured off, and evaporated; the acid residue was dissolved in water, filtered, and neutralized with ammonia, gave a deposit of phosphate of lime, and the double phosphate of ammonia and magnesia. The filtered fluid, when treated with phosphoric acid and ammonia, threw down still more of these insoluble phosphates.

These experiments show that the fibrin of the blood contains, as is usually stated, phosphate of lime, and that the phosphoric acid found in the ash, is not, or at least not entirely, the product of the combustion of the phosphorus ascribed to fibrin. This phosphate of lime which exists in fibrin is little soluble in water, and probably the subphosphate ($3\,CaO, PO_5$). I say probably, because the inquiries of Dr. Heintz,* of Halle, have placed it beyond doubt that the phosphate contained in bones is $3\,CaO, PO_5$; and also, because with the microscope I never found in the fibrin, crystals of ($2\,CaO, HO$) $PO_5 + 4aq$. It is not neces-

* Poggendorff's Annalen der Physik und Chemie. Bd. 77, p. 267.

sary that I should bring this question to decision, because for my inferences I only need the fact that the phosphate of lime is one but slightly soluble. It is said that during life it has been dissolved by means of the albuminous substances; but how has it been dissolved? It is by no means proved that the fluid blood contains $3\,CaO, PO_5$, nor indeed $(2\,CaO, HO)\,PO_5$; but it is obvious that the substances could be dissolved if the phosphoric acid had for its base potash or soda, and the lime another acid, for instance, hydrochloric; or if another acid would combine with a part of the lime, the remainder being united to phosphoric acid, in the combination $(CaO, 2\,HO)\,PO_5$. All that is said with respect to the lime may be also advanced concerning the magnesia.

We have further seen that much more lime and magnesia was extracted from the fibrin, than could have been in combination with the phosphoric acid which was extracted at the same time. No evidence can be brought as to the nature of the combinations in which that lime and magnesia existed, but it is clear that these combinations were little soluble in water, and easily soluble in acids. It is not to be supposed, however, that they were so in the living body, but probably their combinations were more easily soluble in water.

On the other hand, although the pure albumen of Dr. Wurz is soluble in water, yet there are certain albuminates which contain albumen in such a modified state, that it is precipitated if the albuminate is decomposed by an acid. It may be thought possible that the blood contains these albuminates, which, during coagulation, are decomposed by the acids which dissolve magnesia and lime; and the result on the one hand should be, insoluble compounds of calcium and magnesium, and on the other, an insoluble albuminous deposit.

§ 23. The next inquiry to be prosecuted is, therefore, whether fluid fibrin is a peculiar substance, or whether it is formed at the expense of a part of the albumen of the living blood. The plasma sanguinis which was necessary for this inquiry, was easily obtained by bleeding a horse, and receiving the blood in a glass cylinder surrounded by a freezing mixture of ice and salt, not strong enough to freeze the blood, but only for the purpose of hindering the coagulation for some hours, in order to give the corpuscles time to subside; after which the plasma could be gently taken off by means of a long cylindrical pipette. I had observed that blood-plasma whose coagulation had been hindered for some hours by the addition of acetic acid, would not afterwards coagulate if the acetic acid was exactly neutralized by the addition of a few drops of ammonia. This observation led me to the following experiment:—I mixed some blood-plasma with an equal volume of water, to which I had added some acetic acid. Four hours afterwards I added as much ammonia as would nearly neutralize the acid, so that the fluid changed blue litmus-paper to a reddish hue. This fluid did not coagulate at the common temperature of the atmosphere, but when heated to between 140° and 150° F. it became opalescent; and at 160° milk-white, from the coagulation of albumen. Having heated it to 212°, I filtered it; the precipitate differed in no respect from the common albumen of serum coagulated by heat. The clear fluid which had passed through the filter became slightly turbid on the addition of nitric acid or bichloride of mercury, and gave

a precipitate with tannin. But that precipitate was far too inconsiderable to admit of the supposition that the fluid could contain the whole of the albuminous substance which produces the fibrin by its coagulation.

I then diluted the serum of the same horse's blood with an equal volume of water, and added acetic acid until blue litmus-paper was changed to a reddish hue. Then heating slowly in the same manner as before, in a water-bath, a thermometer standing in the serum, the effect was precisely similar to that which happened in the case of the plasma. Also, on boiling, the filtered fluid gave exactly the same reactions with nitric acid, chloride of mercury, and tannin.

In order to avoid all suspicion of fallacy, I prevented a certain weighed quantity of plasma from coagulating, by the addition of dilute acetic acid; four hours after it was nearly neutralized as before, then coagulated by heat, and filtered. Another weighed quantity of plasma was whipped with a hooked platina wire, in order to catch the fibrin as long as coagulation proceeded. When it had finished, the serum was poured off; and the fibrin being well washed with distilled water, this wash was then mixed with the serum, which being very slightly acidulated with acetic acid, was coagulated by heat, and filtered. These two filtrates were then evaporated separately, each together with the whole bulk of the water with which the coagulated albumen had been washed. The residues were dried at a temperature of 260° so long as they continued to diminish in weight, and the weights of each thus obtained in grains were reduced into per-cents. of each corresponding bulk of plasma. The difference between the two was only 0·05 per-cents. In a second experiment conducted in the same manner, the difference was only 0·01.

From these experiments, there could be no doubt that the so-called fluid fibrin, the material for the formation of solid fibrin, had comported itself in the same manner as the common albumen of the serum. In another case, in which I prevented the coagulation of a quantity of plasma by the addition of tartaric instead of acetic acid, the result was the same; that is to say, after being neutralized, it did not coagulate at the common temperature, but at about 160° the whole of the albuminous substances became insoluble; however, on neutralizing the acid, the plasma became more turbid than in the former case. This disadvantage is even more striking with phosphoric and oxalic acids. In some specimens of plasma of horses, the same thing happened even with acetic acid; so that the experiment in these cases cannot be neatly conducted, although the plasma apparently differed in nothing from other samples but in that it became more turbid when slightly acidulated.

Having never known any other difference between fluid fibrin and fluid albumen, than that the one coagulates at the common temperature of the air, and the other only if the temperature is raised to 160°, we have no further inducement to suppose that there exists in the blood-plasma a peculiar substance, so-called fluid fibrin; but we are now compelled to admit that solid fibrin is formed at the expense of a part of the albumen dissolved in the plasma. Indeed, it has already been long known that there are no constant differences in the elementary composition of fibrin and albumen. The results of the elementary analysis of some samples

of fibrin were not more different from those of some samples of albumen, than they were from other samples of fibrin, or than the results obtained from some samples of albumen were from each other. Professor Liebig has pointed out this fact in reference to the fibrin of the muscles and the albumen of the blood. The fibrin of the blood has scarcely even been analysed in the pure state, because it always entangles more or less of organized particles.

It is a commonly received opinion that fibrin contains phosphorus, whilst albumen, according to the recent analysis of Lieberkühn,* contains no phosphorus. But the assertion that phosphorus is a constituent of fibrin, is founded upon the same method and principles of analysis, on which all former chemists have based the assertion that it is a constituent of albumen also. And the method of Lieberkühn cannot be adapted to coagulated fibrin. It would, indeed, be possible to adapt it to the whole albuminous substance of the plasma, but it would then be liable to fallacy, because a part of the albumen passes through the filter. Therefore, some writers discovered the difference between albumen and fibrin rather in their different physical qualities, and their comportment with reagents, than in the results of elementary analysis. But I shall show, at the end of this Treatise, that such distinctions are also of little value.

Our next object will be to inquire how albumen is changed into fibrin.

§ 24. In the prosecution of this part of the subject, the first question which presents itself is, whether the plasma of the blood contains albuminates, which, when decomposed by an acid, give rise to an albuminous precipitate. It has often been debated whether acids hinder the coagulation of the blood or not. In the experiments I have made to elucidate this point, I have not used blood, it being an opaque fluid, but the plasma of the horse obtained in the manner before described. In the description of its conduct with acids, distinction must be made between three cases.

First case.—The acids are added of such quality, and in such quantity, that they would instantly coagulate the serum of the blood. In this case, they of course coagulate the plasma also.

Second case.—The acids are not added in such quantity and quality as instantly to precipitate the albumen, but yet in such quantity as to be considerably in excess. In this case, the plasma does not coagulate any more in the ordinary way, but exhibits different reactions with different acids, as I shall now describe. If there be added to the fresh plasma, mixed with three times its volume of cold water, dilute nitric acid, drop by drop, the precipitate thrown down by the first drops may be redissolved by agitation. If so much has been added as to render the fluid permanently turbid, and it be then boiled, it becomes clear; but on cooling, throws down a copious white precipitate. This is the reaction of the albuminous matter detected by Dr. Bence Jones in the urine, and in a case of Malakosteon.

If the same experiment is made with plasma not previously diluted with water, it coagulates, when boiled, like common albumen.

* Poggendorff's Annalen der Chemie u. Physik, Band 86, p. 119.

If the plasma be mixed with any quantity of phosphoric acid, between $\frac{1}{20}$th and $\frac{1}{3}$rd of its volume, and of a specific gravity of 1·117 (or with oxalic or tartaric acid), it usually becomes slightly turbid, but does not coagulate; after twenty-four hours, however, it will be found to have become a gelatinous mass. If this jelly be placed in a water-bath of the temperature of 212°, it becomes fluid, but on refrigeration, congeals again.

If fresh plasma is mixed with phosphoric, acetic, tartaric, or oxalic acid, and then boiled, it does not coagulate; but on cooling it is changed into a similar jelly to that which, in the former experiments, was produced by the protracted action of the acids. All these reagents produced the same result with the serum of the blood, the jelly being only less firm; because the serum contains less albumen than the plasma, a part of it being transformed into fibrin.

Third case.—The plasma was slightly acidulated with acetic acid, and the effect was, that it became more or less turbid, though often very slightly so. If water were added, it always became more turbid, and sometimes threw down a flocculent precipitate; but if more acid was added, it became clearer again.

No doubt there were here albuminates which, when decomposed, formed a precipitate; but its bulk was very variable, and the serum exhibited the same reactions—for although on the addition of the acid it became less turbid than the plasma, yet when water was subsequently added it became turbid enough. We cannot, therefore, assert that the albuminates which were decomposed in these experiments were the immediate or exclusive material for the formation of fibrin. But I would ask—is it not possible that coagulation may be a process of continual formation and decomposition of albuminates?

§ 25. It next occurred to me to endeavour to produce a substance analogous to fibrin, by the artificial decomposition of albuminate of potash. I prepared some of Dr. Lieberkühn's solid albuminate of potash ($C_{72}H_{56}N_9O_{22}S + KO$),* cut it into pieces as large as a bean, and put them into water, to which I added, from time to time, a solution of $(CaO + 2Ho) PO_5$, in order to keep the reaction always acid. The pieces became more and more milk-white, and began to shrink. At the end of the third day decomposition was complete, and the pieces of albuminate of potash had lost much in volume, and were milk-white, firm, and elastic. Under the microscope it appeared partly amorphous, while a part was delicately striated, and it could be torn more easily in the direction of the striæ than in any other. In water containing one part in a thousand of hydrochloric acid, the pieces swelled into a translucent jelly, as they did also in acetic and phosphoric acid. In a solution of hydrate of potash they were easily soluble, and in ammonia they swelled rapidly, and became translucent. Every one is aware that these are all the reactions which fibrin exhibits, and which it preserves even if dried in the air, or treated with spirits of wine; whereas albumen, when coagulated by heat, neither swells in phosphoric acid of 1·117 sp. gr., nor in acetic acid, nor in ammonia, nor in water containing $\frac{1}{1000}$th part of hydrochloric acid. When swelled by this latter fluid, some pieces of the

* Poggendorff's Annalen der Chemie und Physik, Band 86, p. 117.

mucous membrane of a rabbit's stomach were added, and the decomposed albuminate was digested as easily as fibrin, and much more rapidly coagulated by heat than albumen.

I found, however, some differences between the reactions of the substance I had produced and those of fibrin, but they were differences more of degree than of kind. First, the new substance was more easily soluble in ammonia than fibrin. If I put in two different glass vessels ammonia of the same strength, and placed fibrin in one, and in the other the decomposed albuminate, and twenty-four hours afterwards neutralized the ammonia, the result was always a larger precipitate in the second vessel than in the first. It also appeared that the substance became more translucent in a solution of carbonate of soda than fibrin does. A third point of difference is, that it requires a more concentrated acetic acid to swell it than does fibrin. But in this latter respect differences are exhibited by various samples of fibrin, and the fibrin of horses always swells less in acetic acid than the fibrin of oxen. On the other hand, different samples of the substance I had prepared also showed differences; and I found that it was the more easily swelled in acetic acid, as the solution of biphosphate of lime, ($Ca + 2\ HO$) PO_5, which I used to decompose the albuminate, was more dilute. I soon afterwards learned that the same substance can be as easily prepared by means of phosphoric or acetic acid, as by phosphate of lime. The albuminate of potash is put into a large jar filled with distilled water, mixed with so much phosphoric or acetic acid that it will just turn blue litmus red. When the acid is neutralized by the potash of the compound, fresh acid is added by degrees until the decomposition is completed. For this purpose, hydrochloric acid is less useful, because, when the decomposition has ceased, the slightest excess of acid causes the whole substance to swell up into a translucent, quivering jelly.

Every one who will prepare this substance for himself, and make himself acquainted with its properties, will say without hesitation that it has more resemblance to true fibrin than any other known substance.

I have hitherto directed the attention of the reader only to those points in which it differs from common coagulated albumen and resembles fibrin; but of course it possesses all those properties which are common to both substances. Thus, with nitric acid it produces xanthoproteic acid; with concentrated hydrochloric acid in contact with the air, it makes a violet fluid, &c. I must however make especial reference to the connexions which it has with coagulated casein. It is a well-known fact, that a solution of the albuminate of potash so nearly resembles a solution of casein, that many chemists consider them as identical. Also, that the precipitate thrown down by acetic acid in the solution of the albuminate is rapidly dissolved by an excess of the acid, perhaps because it was precipitated in finely divided particles. With casein it is notorious that the same thing happens; but Bopp* observed further, that the casein precipitated by hydrochloric acid, swells, on the addition of water, to a quivering jelly, which could be dissolved in an abundance of warm water at a temperature of 104° F.

Fibrin, the decomposed albuminate, casein, and the melting jelly

* Annalen der Chemie und Pharmacie, Band 69, p. 16.

produced by the digestion of the plasma or serum of horses with phosphoric acid, are perhaps a series of substances even more nearly connected with each other, than has hitherto been generally admitted.

From what has been hitherto advanced, it will be seen that we cannot admit the existence in the blood of the living body of a peculiar substance which deserves the special name of fluid fibrin,—a name which necessarily presents the idea that it is a substance distinctly differing from albumen and its compounds; and that this fluid fibrin becomes coagulated fibrin by a simple change in the condition of its cohesion. It must, on the contrary, be acknowledged that a part of the albumen of the blood is changed into an insoluble substance—fibrin—which in several respects bears a close resemblance to the insoluble albumen obtained by means of common white of egg, in decomposing Dr. Lieberkühn's albuminate of potash.

The question remains, whether that change is produced in the same manner—namely, by the formation of insoluble albuminates, and their subsequent decomposition.

There can be no doubt that soluble albumen may be changed into insoluble fibrin in various ways with which we are not acquainted, and which no hypothesis explains. Even the fibrin, when separated from the blood, is a very variable substance; but there are two circumstances which seem to point to a formation and decomposition of albuminates. The first is, as I have already mentioned, the quantity of insoluble compounds of calcium and magnesium which are found in every sample of fibrin; and the second is, the contraction of the clot—a circumstance hitherto quite unexplained, and very remarkable, but nevertheless easily understood on the hypothesis that the clot is at first formed by albuminates, which are subsequently decomposed, and therefore have a tendency to contract in the same manner as the pieces of Dr. Lieberkühn's albuminate of potash contract when they are decomposed in acidulated water.

Art. II.

On the Pathology of Cancer of the Stomach. By William Brinton, M.D., Fellow of the Royal College of Physicians, Lecturer on Physiology in St. Thomas's Hospital, Physician to the Royal Free Hospital.

In bringing before the readers of this Review the following Essay on Cancer of the Stomach, it is only necessary for me to premise that I shall follow as closely as possible the method adopted in the article On the Pathology of Gastric Ulcer, which I was permitted to bring under their notice a year ago. The cancerous disease being far less frequent than the ulcerative, I have had, if possible, greater reasons for adding to my own personal experience whatever inquiries amongst Hospital records, Museums, Reports, and Journals could contribute respecting it. And although circumstances have made these researches somewhat less complete than I could have wished, I venture to hope that even where I can add nothing to what is already known concerning this lesion, my results will not be uninteresting to the student of pathology, as being based upon a

broader (and therefore better) foundation of facts than such deductions have hitherto been.

The frequency of the malady I shall not attempt to deduce from mortuary returns; although the writings of Tanchou in Paris, D'Espine in Geneva, and the Registrar-General in this country, would afford a vast array of figures as materials for such a deduction. Unless founded upon necropsies, such returns are useless for pathological inquiries like that we are now engaged in. And though, from the fatal character of the malady, we might with little error regard the number of cases dying from it as exactly corresponding with the numbers it attacked, yet the vagueness and inexactitude of the diagnosis on which such returns are sometimes based, as well as the errors which are frequently made in the mere nomenclature of disease, invalidate all their conclusions. Indeed, to the ordinary uncertainty of diagnosis in general, we must add another special and obvious source of error which would, on the whole, greatly increase the apparent frequency of this particular disease. The frequency and fatality of the gastric ulcer, coupled with the close resemblance its symptoms often exhibit to the cancerous disease of the stomach—a resemblance which it sometimes baffles the most sedulous observation to unmask during life—justify conjecture that, except in returns which accurately distinguish and record the numbers of deaths produced by each of these maladies, the mortality attributed to gastric cancer is sure to be far too great. In like manner, that chronic inflammation of the stomach which is often (and I think most improperly) termed "hypertrophy," may easily be mistaken for cancer; and though a far less frequent source of error, is still by no means unworthy of notice.

Hence it is to large numbers of necropsies, the subjects of which have died from all causes indifferently,* that we are obliged to turn for information on this head. From various sources—but especially from the valuable records kindly placed at my disposal in Guy's, St. George's, St. Thomas's, and St. Mary's Hospitals, I have collected a total of 8468 necropsies, which include 81 cases of primary cancer of the stomach,—a proportion which is about equivalent to 1 per cent., or $\frac{1}{100}$th of the total mortality.

The ratio of deaths from cancer of the stomach to cancer of other organs is one which it belongs rather to the pathology of cancer than of the stomach to determine. Rokitansky† regards the stomach as only less frequently the seat of this disease than the uterus and breast of the female. Lebert,‡ in a promiscuous collection of 447 cases of cancer, gives

* Although not examined expressly with this view, the Hospital Records with which I am best acquainted afford little ground for doubting the substantial accuracy of the above term. Even in the largest general Hospitals, I suspect the mortality from the more frequent diseases—including pulmonary consumption—varies little from the proportions seen in the deaths of the population at large. While the disproportionate numbers of deaths by accident which figure in Hospital Report-Books, perhaps scarcely more than compensate those deaths from "unknown" or "uncertain" causes in the Registrar-General's Returns, which the more accurate diagnosis of the Hospital physician or surgeon renders a rarer explanation in these institutions. In any case, it is probable that such inquiries as those above alluded to would, with proper precautions, rather understate than overstate the frequency of the particular disease they refer to.

† Handbuch der Pathologischen Anatomie, Bd. 1, s. 347.

‡ Traité Pratique des Maladies Cancéreuses, p. 97. Paris, 1851.

the stomach the second instead of the third place in the order of frequency (breast 62, stomach 57, uterus 52, cases), and a proportion of 1 in 8, or 12½ per cent. Tanchou, from the mortuary returns of the Department of the Seine, also assigns the stomach the second place in the order of frequency, but places the uterus first (uterus 2996, stomach 2303, breast 1147): and thus deduces for the stomach a proportion of 1 in 4, or 25 per cent. Lastly, D'Espine found 209 gastric cancers in 471 cancers generally: a proportion (44⅓ per cent.) nearly three times as great as that he attributes to the uterus, and five times as great as that of the breast. The later stages of uterine and mammary cancer would rarely be considered proper cases for admission to a British general Hospital; for the funds of such institutions are with more propriety devoted to the treatment of curable disease, and of infectious maladies in which each person cured probably represents many saved from the possibility of similar dangerous illness. But after allowing for these sources of disproportion, my own researches amongst the necropsies of the above metropolitan Hospitals give me the impression that Lebert's estimate would be nearer the truth, for an ordinary British population, than either of the others. Willigk,* however, who has made a more direct and trustworthy estimate than any of the above, inasmuch as it expresses the numerical results of a series of promiscuous necropsies, finds that out of 184 cases of cancer, 64 (or about 35 per cent.) were cancers of the stomach: while the mammary and uterine cancers amounted to but 12 and 42 cases respectively.

As regards the *age* of its occurrence, I have collected 601 cases which specify this fact. They may be arranged as follows:—

	Between the ages of								
	0 — 20	— 30	— 40	— 50	— 60	— 70	— 80	— 90	— 100
Number of cases of cancer of the stomach . . .	3	32	80	140	162	133	38	12	1

These 600 cases afford an average age of 50 years.

Comparing the above numbers with the numbers of people living at these ages respectively, we obtain an estimate of the relative liability of these epochs of life to the malady. Reducing this to a maximum of 100, we get the following figures:—

	Between the ages of							
	0 — 20	— 30	— 40	— 50	— 60	— 70	— 80	— 100
Liability to cancer of the stomach, taking 100 as maximum .	·227	11·64	31·67	63·005	87·99	100	52·16	59·99

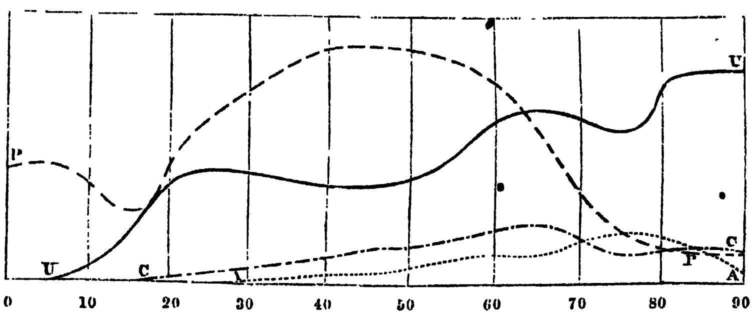
It would thus appear that, before the age of forty, the liability scarcely attains one-fifth of its total amount: in other words, four-fifths of this risk still remain, to be encountered in the succeeding years of life. At the age of sixty, nearly one-half the risk of the malady has already gone by: and at the age of seventy, two-thirds. The small number of cases, as well as of persons living, in the last two decades of life, render the conclusions that refer to these epochs somewhat less trustworthy. But, assuming the accuracy of these estimates, it becomes interesting to con-

* Sections-Ergebnisse, &c., Prag. Vierteljahrschrift, x. 2, 1853; and Schmidt's Jahrbücher, vol. lxxix. p. 90.

trast them with some other diseases of advancing life; and especially with those already compared with the ulcer of the stomach in my previous Essay (January, 1856).

	At ages between								
	0 — 10	10 — 20	20 — 30	30 — 40	40 — 50	50 — 60	60 — 70	70 — 80	80 — 90
Liability to cancer of the stomach	—	$\frac{1}{4}$	$11\frac{2}{3}$	$31\frac{2}{3}$	63	88	100	$52\frac{1}{3}$	60
To ulcer* of the stomach . .	—	20	51	49	47	56	80	75	100
To apoplexy (cerebral hæmorrhage?)	—	$\frac{1}{2}$	$2\frac{1}{3}$	7	16	40	61	100	69
To pulmonary consumption .	47	33	73	92	100	95	65	23	$7\frac{2}{3}$

But instead of chusing for all these diseases an arbitrary maximum of 100, which has a totally different and incommensurable value for each of them, we may profitably attempt a stricter comparison. In the accompanying diagram I have endeavoured to effect this, so that while the varying heights of each curve represent the relative liability of the individual to the particular disease during the epoch of life denoted by the ages (the widths) subjoined, the comparative heights of the different curves also represent the relative liability of the individual to the corresponding maladies at these epochs.†



For example, the line P P in the diagram represents the curve thus calculated for phthisis, or pulmonary consumption: the line U U, that of gastric ulcer: and the lines C C and A A those of gastric cancer and apoplexy respectively. It is the chief advantage of such delineations that they supply at a glance materials for much thought; and deductions out of which no two persons would probably select quite the same for especial notice. But, assuming the accuracy of the calculations on which these

* The reader must recollect that these ulcers differ from the other three lesions, in not being all primary or efficient causes of death. In other words, they are often found in the dead body without our being able to regard them as exerting more than a partial causative influence on the fatal event. (On this point, compare the author's Essays in this Review, No. xxxiii. p. 182; and No. xxxv. p. 160 *et passim.*)

† The deaths by apoplexy and consumption are reduced from the Registrar-General's Report for the year 1847. As mere death-rates, they are of course open to the objection of uncertainty of diagnosis. But errors would to some extent correct themselves, by being in opposite directions. And these maladies are (if we except pulmonary consumption in infant life) likely to be more correctly diagnosed than most others. It may therefore suffice to point out that, while "apoplexy" doubtless includes some cases of epilepsy and renal coma, the cerebral hæmorrhage which forms its most frequent cause would about as frequently end in deaths which are probably registered as "paralysis." Hence it is chiefly the *sudden* deaths produced by such hæmorrhage that are above referred to.

outlines are based, it would follow that, at about the age of seventeen or eighteen there is nearly as great a liability to be attacked by gastric ulcer as to die of pulmonary consumption: and that at about the age of sixty-three, the risk of attack from the former, and of death from the latter, malady, again culminates in the same manner.* While, with respect to gastric cancer, we not only see that the risk of this malady amounts to what is, on an average, barely one-fourth of the ulcer; but that, though much more distinctly and exclusively a disease of old age, its climax or maximum of risk occurs at least twenty years earlier than that of the gastric ulcer. Lastly, comparing gastric cancer with apoplexy, we notice that the risk of the latter disease, beginning about ten years later than that of the former, remains, with little deviation from the proportion of one-half the former, until the liability to cancer has reached its climax; when, by attaining its own maximum in the next ten years, it reverses the above proportion, and at length, between the age of 80 and 90, subsides to a death-risk which is nearly equal for the three very diverse diseases as phthisis, cancer, and apoplexy.†

Sex.—The question of sex is even more difficult than that of age to decide on any sufficient numerical basis. Death-rates are useless, for the reasons already mentioned. The personal experience of private practice is too small; and even if many observers were to group and unite their experience, still its results would be too promiscuous and uncertain. And even in public institutions for the treatment of the sick, the numbers, however large, are useless for all purposes of comparison, unless it be clearly stated what is the average proportion of male to female patients among whom the cases of gastric cancer have occurred.

Such considerations well explain the contradictory results arrived at by different authorities. D'Espine gives 116 cases from the mortuary returns of Geneva, of which 54 are male, and 62 female. But if these cases of supposed cancer included the ordinary proportion of gastric ulcer witnessed in this country, the disproportionate numbers (2 to 1) of females attacked by the latter malady, would explain the above result very differently. Lebert and Louis, who appear to have collected their cases promiscuously, deduce conclusions, the contradictory nature of which is less invidiously explained by the small numbers with which they deal. Lebert gives 42 cases, 19 male, and 23 female; Louis 33 cases, 20 male, and 13 female. Out of 224 cases collected by Dittrich and Willigk, 95 are males, and 129 females. While 233 cases which I have brought together, chiefly from British Hospitals, give 160 males, and 73 females.

The two latter groups of cases, however, are less contradictory to each other than might at first sight be supposed. At least, it is my impression that the London Hospitals receive on an average not less than six males to five females; and hence that the above proportion requires to be reduced

* The interest and the value of this method of pathological inquiry are little affected by the inaccuracies such deductions are sure to contain. Although the author has taken much trouble in the collection of materials, no one can be more sensible of their deficiencies.

† The climax which the risk of gastric ulcer reaches at the extreme age of ninety, seems also (in the diagram) nearly equal to that maximum danger of death from phthisis which occurs between the ages of forty and fifty. Whatever doubt the small numbers of the ulcers at these high ages throw upon this startling conclusion, I must say, that hitherto my clinical researches amongst persons at this period of life have but confirmed it.

to about 140 to 73. While Dittrich expressly states that the hospital at Prague, from which his (and Willigk's?) cases are derived, admits females in far greater numbers than males.

Adding together all the trustworthy returns I have been able to meet with, affords me 784 cases; out of which 440 are male, and 344 female. Such numbers leave little doubt that, whatever the exact proportions of the sexes, the male is the more frequently affected of the two: a fact which it is impossible to avoid connecting with the exclusive amenability of the female to the mammary and uterine localizations of cancerous disease. The above numbers nearly correspond to a proportion of 4 to 3, or 56 to 43 per cent. respectively.

The mode in which age is affected by sex I deduce from 223 cases, which I have collected chiefly from the London Hospitals before alluded to. Of these 151 are males, and 72 females. The epochs of life to which they belong, and the liability they afford when corrected for the numbers living at those epochs, are arranged as follows:—

		At ages between							
		0 — 20	— 30	— 40	— 50	— 60	— 70	— 80	— 90
Cases of gastric cancer	Male	1	8	23	40	30	45	4	
	Female	—	5	12	24	19	8	2	2
Liability to gastric cancer, taking 100 as maximum	Male	$\frac{1}{5}$	$7\frac{1}{5}$	$24\frac{3}{5}$	49	46	100	17	
	Female	—	$4\frac{4}{5}$	$12\frac{3}{5}$	$28\frac{1}{3}$	$26\frac{1}{2}$	$14\frac{3}{5}$	6	22

The numbers in the two last epochs are too small for any safe conclusion. But as a whole, this table of liability is interesting; because, whatever doubt may attach to its details, there can be little risk in accepting its chief conclusions. Thus there are obviously indications of a greater (double) liability of the male up to the age of sixty. And there is still more distinct evidence of a contrast in the epochs of rise and fall of risk in the two sexes. The influence attributed to the few years that follow the close of the fertile (or menstrual) epoch in the female, is contradicted by the precisely equal rise of risk which the same age exhibits in the male. The ten years between sixty and seventy, however, seem to bring about a converse alteration of risk in the two sexes,—increasing that of the male to double, diminishing that of the female to half its former amount. Whether this change really represents a corresponding alteration of risk from cancer in general, or whether it is in any degree equalized by an increased liability of the female to its mammary and uterine localizations, must be left to future researches to determine.

The average age in the two sexes is about 51 in the male, $40\frac{1}{2}$ in the female.*

* A larger number of cases for the above comparisons (394) might have been obtained by adding to the above summary of British necropsies, 160 given by Dittrich (Die Krebsige Entartung des Magens, Prager Vierteljahrschrift, 1848, B. i. s. 1) in his excellent though brief Essay; and 12 by Duchek (Id. op., 1853, i. 8). But I have preferred a limited number of facts to a larger series, not only because they are likely to be more strictly comparable when drawn from kindred sources, but also because (as above mentioned) the Prague Hospital includes a very disproportionate number of females, while the British Hospitals exhibit something like an equality (6 to 5?) of the male and female cases. To this I must again add (what I have already conjectured in a previous Essay), that there can be little doubt that the Prague

The *situation* of the cancerous deposit is specified in 360 of the cases I have brought together. The pylorus was affected in 219 instances—a proportion of exactly 60 per cent., or three-fifths of the whole. The sex of the patients seems to have no influence on the frequency with which the lesion affects this part. Thus, in the 232 British cases, concerning which I have obtained fuller details than the others, the sexes are to each other nearly as 2 males to 1 female (159 to 73). And this group of cases yields 194 which specify the exact site of the cancer; and includes 125 lesions of the pylorus, which are divided in very nearly the same proportion into 86 males and 39 females.

The same group also affords me the basis of some specific conclusions, which are rendered important by the authority of the admirable pathologist whose opinions they somewhat modify. Rokitansky* states that "the cardiac orifice of the stomach is but seldom the seat of cancerous degeneration, and it is a circumstance worthy of note, that the pyloric cancer is exactly bounded by the pyloric ring, and never reaches beyond to the duodenum; while that of the cardia—even where it does not descend from the œsophagus above—always spreads itself over a considerable piece of the œsophagus."

It is only in a very limited sense that we can accept the above word "seldom." For out of the 360 cases already alluded to, no less than 36 are cancers of the cardia; a proportion amounting to exactly 10 per cent. of the total numbers, and to 16⅔ per cent., or 1 in 6, of the pyloric cancers (216). And in the British cases which I have collected, the proportion of cardiac cancers is still larger, namely, 25 in 194, equal to 13 per cent., or 1 in 7 of the whole number; and 1 in 5 of the pyloric lesions. From these cancers of the cardia, I have excluded all cases except those in which the orifice itself was either solely or chiefly affected. And although it is possible that some of them may have been originally developed around the œsophagus, and only extended to the stomach, yet this objection, which applies equally to all the above cases, can scarcely be regarded as a frequent explanation in the absence of express evidence to that effect. We may therefore conclude that cancer of the cardia is *not* an unfrequent variety of this lesion in the stomach.

It is still more curious to notice how specifically the observations I have brought together contradict the next proposition of the above eminent pathologist quoted above. The 125 cancers of the pylorus included no less than 10 cases in which the disease was *not* bounded by the valve, but passed beyond it for a variable distance (often an inch or two) into the duodenum. Lebert† gives another instance of the same kind: making in all eleven exceptions to this supposed absolute and universal rule.

To the equally absolute statement, that the cardiac cancer *always* involves the œsophagus, I have also found two conclusive exceptions. Allowing for the much smaller number of these cardiac cases, it is not impossible that such exceptions may be almost as numerous as the preceding. Indeed the proportions in the cases I have collected, exactly

Hospital (like many of the German Krankenhäuser) would probably differ widely from our own hospitals in the number of aged persons it receives and contains: being in this respect much more akin to a workhouse infirmary than is an ordinary British metropolitan hospital.

* Handbuch der speciellen Pathologischen Anatomie, Bd. ii. s. 205. † Op. cit., p. 467.

correspond to such a conjecture; these exceptions being 11 in 159 pyloric cancers, and 2 in 30 cardiac cancers (or about 1 in 15 for each).

The rules, therefore, which Rokitansky has the merit* of having laid down, in these respects are (like many others in this branch of natural science) of general, but not universal, import. But their value is not much affected by occasional exceptions. For their significance, far from depending on any mere numerical ratio of 15 to 1, is much more essentially measured by the antagonism of the fact in the two classes of lesion —the limitation, and the spread, of the cancer beyond an analogous boundary or extremity of the organ. Hence it becomes interesting to inquire whether any reason can be assigned for it.

On the whole, the present state of our knowledge does seem to justify a conjecture of this kind. The character of the above laws—general, but not universal—sufficiently hints that they do not depend on any direct or single cause, such as would necessarily be of universal operancy, but rather on some secondary causation, which only indirectly brings about its result. And the first and nearest inquiry is one bearing on the anatomy of the stomach in relation to that of the cancerous deposit. The question naturally suggests itself—"Is there nothing about the structure of the pyloric and cardiac orifices which, by opposing or facilitating the continuity of the cancerous deposit in their tissues, respectively limits or permits its spread to outlying parts of the alimentary tube?"

The answer to this question appears to afford exactly the kind of explanation suggested. The minute anatomy of gastric cancer conclusively shows (as we shall presently have occasion to mention in detail) that the morbid cell-growth is generally first deposited in the sub-mucous areolar tissue; that it thence advances (by means of those partitions of the same tissue which separate and ensheathe the bundles of the organic muscle) into the subjacent muscular coat; that, at a later period, it engages the mucous membrane, by disorganizing which it soon produces symptoms of grave (if not fatal) import; and lastly, that the complete implication of the peritoneal coat is a still later phenomenon, and therefore is, in the main, generally anticipated by death.

Now it is precisely in the degree and kind of continuity by which these first and chief seats of the cancerous deposit—the sub-mucous and muscular tissues of the stomach—merge into their analogues in the adjacent segments of the digestive canal, that the cardia and pylorus differ most remarkably from each other. The cardia is so organized as always to concede to whatever food may be swallowed a transit in the onward direction,—a transit during which its contraction implies a muscular movement that is directly continued† from the œsophagus into the stomach. In accordance with these requirements, there is the most perfect con-

* The reader will hardly think me invidious in calling attention to any inaccuracies in a work so widely known and appreciated as the great Text-Book of Pathological Anatomy cited above. The less so, perhaps, that few books of the present day demand or repay so thoughtful and minute a study. It is because every sentence evidently sums up a wide series of accurate observations, that we may justifiably apply to it a criticism of unusual severity: a criticism which, even if it weighs every word, will scarcely do more than the author's terse and weighty propositions really deserve.

† For fuller details on this and other points connected with it, the reader may be referred to the article STOMACH, by the Author, in the Cyclopædia of Anatomy—Supplement, pp. 311 *et seq.*

tinuity between the sub-mucous and muscular coats of the two segments. The latter, in leaving the œsophagus, radiates its longitudinal layer on all sides, and with special distinctness along the lesser curvature; while its circular or transverse layer has an almost equal continuity with both the circular and oblique layers which represent it in the stomach.

On the other hand, the pylorus is constructed so as to resist the onward transit of the contents of the stomach by its powerful contraction during the whole period of gastric digestion—a contraction that appears to be slightly over-balanced by every wave of the powerful peristalsis which gradually engages the strong muscular layer of the pyloric region, so as to strain off a little of the more liquid contents of the stomach during each of these undulations. The violent action into which the pylorus thus passes from the time of entry of food into the stomach, requires its complete isolation from the adjacent duodenum. And a careful dissection of the pylorus shows that this is attained, to some considerable degree, even for the sub-mucous areolar tissue, by its small quantity, and its close and dense attachment, at the line of junction of the two mucous membranes. But it is especially in the muscular structure that this isolation is most distinct. The mode of attachment of the duodenum to the stomach may be best expressed by the statement that, instead of being continuous with the pyloric extremity of this organ, it is attached around it, at a short distance from its termination,—in short, that the commencement of the bowel receives the stomach, just as the vagina receives and embraces the neck of the womb at some distance* from its mouth. And hence not only is the continuity of the muscular coats of the two organs diminished to the junction of the thin linear layer of the duodenal coat, but this delicate layer comes off from the stomach at an angle, and at a distance from its pyloric extremity, which render it far more likely that the deposit should engage the massy pyloric valve beyond this attachment, than that it should diverge—through what seems to be chiefly a second or outer process of areolar tissue—towards the duodenum. Indeed such a view receives a frequent confirmation in the ordinary phenomena of the distribution of cancer; in the way, for example, in which sub-peritoneal cancer almost always involves the continuous membrane that covers two or three organs or segments of the digestive canal; or the converse frequency with which the disease, when primarily affecting a gland, leaps over, as it were, a scanty medium of areolar tissue, to fix on a more congenial organ at some distance from its original seat.

It may be interesting if we sum up the other situations of the above 360 cancers of the stomach, by a table, comparing them with as many ulcers of this organ—the more so, indeed, that some authors appear to regard these two diseases as affecting the pyloric and cardiac extremities of the stomach in nearly equal proportions.

* I am aware that the illustration I have selected may seem to militate against the very conjecture for which it helps to lay the foundation: by showing an equally discontinuous structure, where, nevertheless, cancer *does* often spread. But we must recollect that, owing to the less direct influence exercised by these sexual organs on the life of the patient, the cancerous lesion is prolonged to a much later period; to one which, at any rate, might well suffice to convert the ratio of continuity of cancerous deposit seen in the stomach (1 in 15 cases), to that which seems to prevail in the uterus (1 in 3).

	Total.	Pylorus.	Lesser curvature.	Cardia.	Stomach generally.	Greater curvature.	Posterior surface.	Anterior surface.	Middle segment.
Number of examples of gastric cancer.	360	219	38	36	13	11	11	11	4
Number of examples of gastric ulcer.*	360	52 (25?)	98	(5?)	0	8	177	18	0

Such a comparison of the two diseases shows that the remarkable preference of the cancerous lesion for the orifices of the stomach (which together form the site of 71 per cent., or nearly three-fourths of the gastric cancers), is not at all shared in by the ulcerative disease. In the recorded necropsies of the latter, indeed, the terms "pylorus" and "cardia" so often refer to the mere neighbourhood of these valves, that a large deduction (probably at least one half, as indicated by the notes of interrogation in my table) must be made from the moderate proportion (16 per cent., or less than one-sixth) which their numbers would together imply.

The contrast of the remaining situations does not seem to call for any comment. Of course, the absence of all cases of ulcer of the whole stomach, or of the whole of its middle region, is sufficiently explained by the death that necessarily intervenes as soon as the process of ulcerative absorption has destroyed a large proportion of a mucous membrane so essential to life as that of the stomach. I may add, that I have taken every care to exclude from these cancers of the whole stomach, those cases of chronic inflammation of the gastric parietes sometimes mistaken for scirrhous disease—have generally, for instance, either obtained direct evidence derived from its microscopic or anatomical characters, or indirect evidence equally trustworthy in the shape of the presence of secondary cancer in other organs.

The *anatomy* of gastric cancer is most conveniently subdivided, according to the three chief forms of this deposit which it affects—the scirrhous, medullary, and colloid cancer; to which we may add a fourth, the villous cancer of the mucous membrane.

Of these varieties of cancer, the scirrhous is by far the most common. Out of 180 cases which name the species of cancer present, 130 (a proportion equivalent to about 72 per cent., or nearly three-fourths of the whole) belong to this variety. The same group affords 32 instances of medullary cancer, 17 of colloid, 3 of melanotic pigment, and 1 of villous cancer. These numbers about correspond to per-centages of 18 and $9\frac{1}{3}$ for the medullary and colloid cancer respectively; or, in fractions, to rather more than one-sixth and one-twelfth. The melanosis was in one case diffused generally over the whole body, in the form of small tumours; in the two others was such a superficial colouration of the gastric mucous membrane covering the cancerous tumour, as would scarcely merit this title, unless substantiated by very careful histological examination.†

As my limits oblige me to confine myself strictly to the local or gastric features of the malady, I am spared the necessity of dwelling on the his-

* These cases (rearranged, and raised by multiplication from 220 to 360) are derived from the sources specified in the author's Essay in this Review for January, 1856.

† In order to prove that it was not due to any of those alterations in colour which mere ecchymoses in this situation so often undergo from the action of the digestive fluids.

tology of these three forms of cancer. The more so, indeed, that they merge into each other by gradations of almost infinite variety. For not only may we regard the scirrhous, medullary, and colloid cancer of the stomach as being what they are in other parts of the body—manifestations, in a different form, of one and the same disease; but as constituting, with even greater frequency, mere consecutive phenomena of one and the same morbid process. In other words, in a great many cases, what is originally scirrhus becomes admixed with more or less medullary or colloid cancer, if it be not absolutely metamorphosed into it. While from collateral circumstances, we are entitled to conjecture that in many more a similar complication would occur, but for the occurrence of death in an earlier stage of the disease. Whatever may be the apparent temerity of such a view, it does but state, in a somewhat circuitous way, a proposition the truth of which is obvious at first sight—namely, that our knowledge of the pathological anatomy of this (or any other) disease is built up from a number of observations; and that of these observations, many of the most valuable in respect to the origin and succession of morbid changes in the organ that forms the seat of the disease, are only afforded us either by casual deaths, by intercurrent or secondary maladies, or finally by an effect of the disease on the powers of life which is so complex and variable, that we may fairly view it as somewhat independent of the local mischief.

In the vast majority of cases, the cancer begins by a deposit in the submucous areolar tissue. This loose but thick layer—which, by intervening between the mucous and muscular coats, shares in every movement of the two, and especially concedes and limits that free play of the mucous membrane by which the passive contraction of the muscular coat throws it into folds, to be effaced by any distension of the stomach—is composed of its ordinary constituents of white and yellow fibrous tissue, the elements of the latter being of large size, and in great quantity. And the first rudiments of a cancer are generally deposited among these fibrous structures in the form of a dense knot, of a dull white colour, and a firm and hard (rather than tough) texture. This opaque mass includes the normal fibrous elements of the tissue, but in such an intimate state of fusion that it is almost impossible to detect them in any quantity. Indeed, their proportion is so small that, even allowing for the mechanical difficulties that oppose their isolation, it is difficult to avoid the conjecture that they are compressed and killed by the new growth; in which, so to speak, they remain dead, as well as buried. And it is to the strictly retrograde changes of this kind which these healthy original structures undergo, that I am disposed to attribute the fatty molecules which are found in even the freshest specimens of scirrhus, and in the earliest stages of its growth. In many cases, at least, I believe this fat to be produced by the mere decay of the ordinary tissues.

Blastema in any quantity is rarely met with, save as an element of the whitish juice which exudes from the scirrhous mass when it is squeezed or scraped. The irregular warty character of the layer into which these knots soon expand, seems to be often quite unconnected with any peculiarities in the arrangement of the scirrhous growth with respect to the plexuses of vessels that occupy the sub-mucous areolar tissue; though occasionally the protuberances seem to be received into vascular loops or

meshes of this kind. Finally, in most instances, however small or recent the mass of scirrhus, a large proportion of its substance consists either of fibres, or of cells, the fibrous extremities of which are so long as almost to deserve this title. Mere nuclei are in but small proportion.

It is often difficult to determine by examination whether the scirrhous deposit has originally predominated on the muscular or external aspect of this layer of sub-mucous tissue. But there are good grounds for supposing that this is generally the case. At any rate, it is in this direction, and towards this constituent of the stomach, that its progress in the succeeding stage of the disease is almost exclusively directed. The sub-mucous and muscular strata become inextricably fused into each other at their line of contact; while as yet the mucous membrane itself is separated from the disease by an interval of healthy areolar tissue. The muscular coat is then transformed into scirrhus by an extension of the same process as that by which the sub-mucous tissue was itself at first affected.

The appearances of the scirrhus in this stage are too specifically connected with the organ it affects, to be altogether passed over. In rare instances, we may find a dense white semi-translucent mass, of tolerably uniform structure, occupying the whole thickness of what was formerly the muscular coat, and quite undistinguishable from the original focus of the disease in the sub-mucous areolar tissue.

But in the majority of cases, the cancerous deposit is much less homogeneous, and exhibits characters which may almost be regarded as relics of that differentiation of tissue present in the healthy textures which it has replaced. On cutting through the diseased mass, we find that the deeper portion which has replaced the original muscular substance, exhibits two varieties of tissue, so arranged as to give it more or less of a honeycombed appearance on section. There is a brownish, reddish-yellow, or at any rate darker and somewhat softer mass, enclosed in small polygonal meshes formed of whiter and denser tissue. Of these meshes, which are irregular in shape and size, those are in general strongest and thickest which run transversely to the axis of the stomach from the sub-mucous, to the sub-serous areolar tissue. They are evidently the transformed septa of the bundles of fibre-cells which unite to form the thick muscular coat of this part of the stomach; and they inclose cavities which, originally occupied by these muscular bundles, now contain, in addition to their fatty and decayed relics, a cancerous deposit which is often visibly less fibrous, and richer in nuclei and blastema, than that of the septa themselves. Their arrangement, however, does not always precisely recal that of the original sheaths of the unstriped fibres. The thickest septa are, as above mentioned, often directed transversely to the mucous surface; and, from the direction of the circular fibres, are best seen by cutting lengthwise through the mass in the same plane. But irregularities in the deposit of the cancerous matter, as well in the septa as in their inclosed cavities, easily affect the uniformity of this arrangement, so as to render the meshes comparatively larger or smaller, or even to obscure and obliterate them altogether. And as a pretty equable deposit around the narrow pyloric end of the stomach is one of the commonest varieties of scirrhus in this organ, the first effect of the commencing disease often presents an obstacle to further

growth in the direction of the centre of the tube, such as even aids the natural tendency of the disease to spread outwards; so that the septa seem to diverge (or even radiate) in passing from the centre to the periphery of the tumour, and include an increasing amount of their darkish or gelatinous-looking contents as they approach towards the tissues of the peritoneum.

There is one circumstance which renders it especially important to appreciate these characteristic appearances of the gastric scirrhus in this stage of its growth. It is probably not uncommon for specimens which present little more than the average distinctness of this differentiation seen in ordinary scirrhus, to receive the name of "colloid" cancer. And though it is true that in many instances of the kind a careful and accurate histologist could distinguish the tissue as really fibrous or scirrhous cancer —a decision in which its physical as well as microscopic characters would alike concur—still in some cases, in which the meshes are large, and enclose much fluid exsudation, it is not very easy to say to which of the two species the cancer really belongs. To some examples, indeed, it would be very difficult to deny the appellation of colloid. At any rate we may assert that a large proportion of so-called colloid cancers of the stomach are either essentially scirrhus, or are developed out of a cancerous deposit which was originally of this species. And hence that the ordinary way in which this name is at present used, obliges us to deduct from the recorded cases of colloid cancer of the stomach a very considerable percentage as modified scirrhus; and (what is even a more practical point for the pathologist) to regard such cases as having little or no weight in the decision of an important problem in the natural history of gastric cancer—namely, the average duration of the three forms of the malady, or the rapidity with which they severally destroy life.

The further progress of the disease conducts it to the mucous and serous surfaces of the stomach; still, as it goes, fusing into itself, and confounding together, the structures previously present.

In the serous membrane, the tenacity of its tissues, as well as its pathological tendency to inflammation, soon give rise to adhesion of the cancerous segment of stomach to some neighbouring viscus. How far the cancer itself lends the aid of any specific irritation to this process it is difficult to estimate: although, from the analogy of the ulcer, one may suspect that we have no great need to assume such an explanation. Of course the particular portion of the tumour thus attached, as well as the viscus to which it is fixed, are subject to considerable variety. But while (for obvious reasons) the adhesion of the ulcerous stomach accurately corresponds to the site of the lesion, and to the viscus which is normally in contact therewith, that of the cancerous organ appears to be regulated by circumstances less obvious. The diaphragm, liver, pancreas, and spleen, constitute the most frequent sites of such adhesion; and in nearly the above order of frequency. In short, it is at the upper and posterior aspect of the stomach, and with a frequency that is quite disproportionate to the predominance of the cancer itself in these positions, that this fixation chiefly occurs. It is probable that the chief cause of this peculiarity is to be found in the relative immobility of the organs which occupy this part of the cavity of the belly; and that, so far, it is

analogous (if not strictly parallel) to the infrequency or tenuity of adhesions between an ulcer of the anterior surface of the stomach and the corresponding wall of the belly. Indeed, the chief difference seems to be, that the contrast, which we can only verify in different cases of ulcer, is often illustrated by opposite surfaces of one and the same cancerous tumour.

In advancing towards the mucous membrane, the disease inaugurates a series of changes which, however secondary in their essence to the deposit that has long preceded them, are in the majority of cases probably more intimately connected with those symptoms which attract the notice and claim the skill of the physician, than any other of the numerous details which the morbid anatomist has to study.* The destruction of the gastric mucous membrane not only directly involves the partial ablation of an organ essential to life, but leads to a train of indirect results of at least equally serious import. The certainty of hæmorrhage and ulceration; the probability of a grafting of cancerous germs into distant organs by means of the current of the lymph or blood; together with an increased possibility of obstruction, dilatation, and hypertrophy of the stomach; or of fistula opening into other parts of the canal, the chest, or the surface of the belly:—all these risks are now superadded to those of the cachexia which the cancer itself expresses, and to those of the cachexia which it can produce.

As the cancer approaches the immediate proximity of the mucous membrane, its first effect often appears to be a slight though perceptible increase in the thickness and firmness of this membrane, such as a mere increase of its healthy nutritional fluid could produce. It resembles, in short, a stomach taken from a younger and healthier person, or from a fresher corpse, than that to which it belongs.* Then follows a fusion of the cancer with the under surface of the mucous membrane, giving to this latter a complete immobility upon the subjacent textures, and a dull, white, thickened appearance. The local anæmia which this state seems to imply, is probably often due to a complete occlusion or compression of the blood-vessels of the membrane: a state which, by the pressure it throws on the obstructed current of the blood, occasionally appears to give rise to an hæmorrhage strictly analogous to that which so often causes death in cirrhosis. And it is probable that this interference with the vessels (quite as much as any specific tendency of the cancer itself) is answerable for the destructive changes that now ensue in the membrane. This destruction, whether effected by the intermediate stage of dark pulpy or ashy softening that merges into an ulcer, or by the more rapid death of the tissues piecemeal by a process of sloughing or gangrene:—in any case speedily leads to the same result, to the production of a solution of continuity, which is bounded exclusively by the exposed or denuded cancerous growth.

The subsequent phenomena still permit of considerable variety in different specimens. On the whole, the most frequent change is that

* And may be compared in so far to the stomach of diabetes: which, as I described many years ago, from a specimen I examined for Dr. Todd, is best summed up as remarkably healthy, and unusually resistant of cadaveric changes. Subsequently, I believe, Dr. Todd and myself were both misquoted to mean that the tubes were in a state of abnormal distension.

in which the periphery of the cancerous mass, and especially that part of it which is denuded by the removal of the mucous membrane, becomes the seat of an infiltration of medullary or areolar cancer. This new deposit generally protrudes into the cavity of the stomach by spongy masses of variable size and shape, which constitute what is (κατ' ἐξοχήν) a true *fungus hæmatodes*: a soft pulpy fungus, that sometimes breaks down by a process, half sloughing, half suppuration (*verjauchung*); but oftener associates with a similar process such an amount of hæmorrhage as materially to disguise and conceal it. Hence, unless to a strict scrutiny (or even a careful miscroscopic examination), there is often no perceptible difference between the coffee-grounds vomit ejected in such cases during life, and that expelled from the merely ulcerous stomach. And in like manner the surface and edges of the cancerous fungus itself are sometimes occupied by coagulated and altered blood, that bears no testimony whatever to the character of the lesion which has procured its extravasation.

This secondary deposit of medullary cancer seems to be precisely analogous to that secondary form of colloid already mentioned, than which it is, on an average, more frequent, and to which it not unfrequently serves as a basis. Both of them suggest the question as to how far they are real transformations of the original scirrhus, how far they are merely admixed with it. But the morbid anatomy of a large number of cancerous stomachs probably indicates a somewhat different answer for each of the two. If we are to accept the term "colloid" in its ordinary sense, it will certainly be difficult to exclude from this category cases in which it seems to have been developed, by something very like a genuine transformation, out of both scirrhous and medullary cancer. In the former of these two species we have seen that it occasionally (though very rarely) appears to perpetuate a normal differentiation of tissue; that it is the morbid analogue (so to speak) of the sheaths and bundles of the unstriped muscle.* In the latter it amounts to the introduction, into a comparatively homogeneous cell-growth, of an exactly similar differentiation: describable in most cases as a formation of delicate balks or partitions of fibres, which are arranged so as to enclose irregular cavities containing a softer mass of cells or rudimentary fibres. On the whole, the ordinary situation of the deposit (whether medullary or colloid) seems to show that it is rarely or never produced by any genuine transformation of the pre-existent scirrhus; and that though it is not at all improbable that a moderate interstitial deposit of new cancer often admixes a certain quantity of medullary or colloid with even the deepest parts of such a scirrhous tumour, no real conversion of the previous scirrhus ever takes place. At any rate, we have no evidence of such a process; and all the softening that

* It may be objected, that definite instances of this kind are too rare to justify the confusion of terms that would arise from calling them colloid. The validity of such an objection I quite admit. But I think it would be impossible for any one to analyse and study the records of even modern pathology, without coming to the conclusion that (rightly or wrongly) the name "colloid" cancer of the stomach is often applied to a very moderate exaggeration of an appearance which few cases of scirrhus of this organ are altogether devoid of. Hence I offer the above remarks chiefly as a clue to these records, and as a hint with respect to the physiology of cancer; though I should be happy if they called attention to a vagueness (if not inaccuracy) from which it is but justice to our German fellow-labourers in the field of Pathology to say they seem comparatively free.

occurs in the scirrhus itself seems quite explained by the degeneration of those healthy tissues which it has enclosed within its mass, and by the true cancerous softening to which it is liable, in common with all other forms of this adventitious growth.

With respect to the origin and growth of the two other and rarer varieties of gastric cancer, in their uncombined form, I have little to add (certainly nothing material) to the excellent account given by Rokitansky,* and to Dittrich's† valuable and original commentary upon it. I think, however, that it might be laid down more definitely than they have done, that the medullary deposit, as a rule, begins more immediately beneath the mucous membrane, and the colloid beneath the serous membrane, than does the ordinary scirrhus or its combinations. But the exceptions to this rule are so numerous as to deprive it of much claim to our notice.

The villous cancer of the stomach seems to be strictly a deposit at or near the basement membrane; not merely (as has been suggested) an isolated medullary deposit in the sub-mucous areolar tissue growing by continual accretion on its mucous side, and thus sometimes enlarging to a tumour here, of which the original basis remains a mere peduncle or stalk—but rather a cancer of the mucous membrane itself, the very proximity of which is the chief cause of its undergoing so great a change of form, while it suffers so little direct injury to its structure. That the deposit is, in the majority of instances, *within* the basement membrane, the microscope leaves little reason to doubt; a proposition which, if true, establishes an important distinction between this and the epithelial cancer which has been sometimes regarded as its analogue.

It is only in conjunction with the preceding remarks that we can accept or interpret numbers like those I have already mentioned, in alluding to the frequency of the three forms of cancer in the stomach. In point of fact, the numbers given probably represent little more than instances in which the corresponding form of cancerous deposit formed a large or predominant portion of the whole mass. Of the 32 instances of medullary cancer, for example, it is probable that several at least were merely large combinations or admixtures of this growth with what was originally and essentially a scirrhus. And of the 17 cases of colloid, I suspect even a larger proportion ought to be referred to a similar category. This conjecture is confirmed by the observations of Dittrich,‡ who found only 3 out of 11 colloid cancers to be pure and uncombined examples of this variety; the remaining 8 being combined, 7 with scirrhus, (2 of these also with medullary cancer) and 1 with medullary cancer.

The numbers I have collected include altogether 34 examples of colloid in 417 cases of gastric cancer. If the proportion observed by Dittrich were applied to these, it would reduce the above number (which about equals $8\frac{1}{8}$th per cent.) to 9 cases of pure alveolar cancer, equivalent to a proportion of $2\frac{1}{8}$th per cent.

The destructive process which ultimately engages the free or internal aspect of a gastric cancer, after its mucous membrane has been removed by the processes already mentioned, is of especial interest, from its close

* Op. cit. † Op. cit. ‡ Op. cit., p. 22.

connexion with the appearances by which the cancerous and ulcerous stomach sometimes resemble each other.

Perhaps the commonest method by which cancer simulates ulcer of the stomach, is that sometimes brought about in the course of the process described by Rokitansky; in which, generally with little or no previous formation of medullary or areolar cancer, the scirrhus, denuded by ulceration or sloughing of the mucous membrane, gradually sloughs in round circumscribed patches, so that its tissues exfoliate in successive strata, and thus excavate tolerably smooth pits or fossæ in the crude cancerous mass. Here, however, the distinction is rarely by any means difficult. The bottom of the pit remains more or less sloughy; or even if this characteristic appearance is disguised by a casual hæmorrhage, such as might equally complicate an ulcer, still the quantity of the surrounding mass, and especially its histological characters as seen under the microscope, would rarely allow any room for doubt. Besides, in the ordinary ulcer, thickening is scarcely compatible with sloughing; and is almost always associated with a history that sufficiently refutes the notion of malignant disease.

But while there can be no doubt that in the majority of cases a careful examination would scarcely allow any room for hesitation as to the ulcerous or cancerous nature of a given specimen, it seems probable that there are rare instances, in which nothing that the most sedulous examination of the dead body could reveal, would justify a decision. The smooth circular excavation of a scirrhus may obviously imitate the ulcer surrounded by a hard and thickened mucous membrane. If the cancerous hollow be for the time denuded of slough, or covered with blood, we lose another of the means of distinction that a mere inspection could afford us. Would the physical and microscopical characters of the hardened periphery of the sore always decide the question? I am afraid not. In other words, though my own limited experience has never yet left me in doubt, I have known instances in which the dense cicatrix around an ulcer has yielded portions respecting which, had I seen them separately, and been obliged to decide their character solely from their appearance, their incision, and even their microscopic anatomy, I should have had great difficulty in coming to a decision. In respect to the microscope, indeed, the great variations in the quantity of cell-growth contained in a scirrhous tumour, and in the developmental stage attained by its fibres, will sometimes render its diagnosis from the more complete forms of cicatrix-tissue (containing a fair proportion of fibres and long fusiform cells) anything but an easy task.

Such equivocal cases have suggested themselves in a still more marked form in some of the numerous records and specimens to which my researches have led me. This form, however, I would rather regard as a contingency, than definitely announce as a fact. Is it not possible, I would ask, that the sloughing or ulceration of a limited deposit of scirrhus may not only imitate a mere ulcer, but produce or become one? May it not happen (if only in infinitely rare cases) that the destructive process destroys all the malignant deposit present, leaving behind it neither more nor less than a circular ulcer, bounded by healthy, or at most inflamed, tissue?

(To be continued.)

PART FOURTH.

Chronicle of Medical Science.

ON THE DEVELOPMENT AND STRUCTURE OF THE TEETH OF MAMMIFERA.

BY DR. ADOLPH HANNOVER, OF COPENHAGEN.

(*Translated by* T. WHARTON JONES, F.R.S.)

THE following is a translation in abstract of Dr. Hannover's admirable monograph, Ueber die Entwickelung und den Bau des Säugethierzahns, recently published in the 'Nova Acta Acad. Cæs. Leopold. Nat. Cur.,' vol. xxv. part ii.

In the mammifera, the dental sac contains four distinct elementary structures.

1. *The Dentine Germ.*—This is a soft body at the bottom of the sac, which early presents the permanent form of the crown of the tooth, and is, by a process which Dr. Hannover names *dentification*, converted into the dentine or ivory of the tooth.

2. *The Enamel Germ.*—This immediately invests the dentine germ, and consists of cells vertically disposed, which are at first soft, but afterwards converted by the deposition of earthy matter into the hard columns composing the enamel.

3. *The Cement Germ.*—This, the outermost of all the structures of the dental sac, is, by a process of true ossification, formed into the cement or bony substance of the tooth.

4. *Membrana Intermedia.*—The cement germ is not in immediate contact with either the enamel or the dentine, but is separated from them by a particular membrane, which, at the crown of the tooth, has attached to its inner surface the enamel cells, and consequently is interposed between the cement germ and the enamel; whilst, at the root of the tooth, where the enamel germ ceases, the membrane in question lies between the cement germ and the dentine germ. This membrane, which Dr. Hannover names *membrana intermedia*, and to which he considers due attention has not been hitherto directed, is found, in the fully-developed tooth, metamorphosed into the *stratum intermedium*.

PART I.—HISTOLOGICAL HISTORY OF THE DEVELOPMENT OF THE TEETH OF MAMMIFERA.

1. *Dentine Germ.*

In the new-born infant, the dentine germ of the permanent incisor tooth consists of a homogeneous, reddish-yellow, semi-transparent substance. The edge of the dentine germ being older than the base, the earliest conditions are best seen, the nearer the base the germ is examined.

At the base the germ consists of very small cells, *dentine cells*, deposited without any definite order in a transparent intercellular substance. This being in small quantity, the cells lie closely pressed together, and are thus rendered partly angular. The nucleus, which is very little smaller than the cell itself, is rather dark, coarsely granular, round, oval, or angular.

Whilst the cells of the dentine germ are in this very early stage towards the

base, their development is already much further advanced in the free dark border of the same germ.

The first change perceptible in the cells of the free border, consists in their elongation and arrangement in rows, one behind the other. This change is not perceptible in the cell membrane, on account of its transparency, but is very distinct in the darker nucleus. The nuclei thus become more slender, and four to five times longer than before, lie, on the whole, parallel with each other, and are disposed perpendicularly at the border of the germ. From the ends of the elongated nuclei there are fine prolongations, which appear light or dark according to the change of the illumination. Dr. Hannover has not been able to determine whether these be prolongations of the nucleus or cell itself, but from analogy, thinks the latter more likely. The prolongation of the posterior end of one cell coalesces with the anterior prolongation of another; and in this coalescence of the prolongations and processes, the *tubes* of the *dentine*, together with their ramifications, originate.

When the nucleus is at last fully drawn out in its length, the distinction between it and its prolongations no longer exists. The nucleus forms the lumen and the contents of the permanent dentine tube; the walls of which, on the contrary, are formed of the cell membrane and cell contents.

When the tubes have been thus formed, they become hardened by the deposition of earthy matter. This process, which Dr. Hannover names *dentification*, takes place very soon after the first appearance of the germ, and commences in the oldest part or the free border of the germ, so that the cell series in the border may be found dentified, whilst the back-lying layer is still in a soft state.

Dentification, or the deposit of earthy matter, takes place first around the lumen of the dentine tubes, later in their walls, and in the intercellular substance.

It is to be noted that the lumen of the tube is very often looked upon as the tube itself, whilst the walls of the tube have been altogether overlooked, or mistaken for interspaces between the tubes.

The contents of the tubes are, in the fresh state, limpid like water, fluid, and of the same nature as in the still soft prolongations of the cells; yet, in all probability, impregnated with earthy salts quite as early as the walls of the tubes.

Contemporaneously with the dentification of the tubes, proceeds the solidification of the transparent intercellular substance. It appears in this state structureless and clear, but somewhat darker than the walls of the dentine tubes. In the hardened state it merits the name of intercellular substance.

From the preceding demonstration, it is seen that the dentine tubes have their origin immediately in the coalescence of the dentine cells; and that there is nothing like a secretion of dentine by the germ. This being the case, the ground for the admission of a particular *membrana præformativa* is taken away.

2. *Cement Germ.*

Although the enamel attains its full development earlier than the cement, Hannover has found it more convenient to give the description of the cement germ before that of the enamel germ.

The nature of the cement germ has been mistaken by many of even the most recent observers, because they have incorrectly attributed to it a part in the formation of the enamel. The cement germ has nothing to do with the formation of the enamel, and is, indeed, wholly separated from it by a particular membrane —that called by Hannover *membrana intermedia.*

The formation of the cement or bony part of the tooth takes place through the cement germ in a manner peculiar, and hitherto unknown. Whilst the dentine tubes are immediately developed from the dentine cells, the cement, on the contrary, attains its permanent form only by a threefold process. In the first stage of development, the primordial cells, of which the cement germ consists, change completely into fibres; in the second stage, true cartilage cells form in the now fibrous primordial germ; the third stage is that of ossification.

For the investigation of the earliest conditions of the cement germ, Hannover has found the cement germ of the permanent incisor of the new-born infant likewise the best adapted. The cement germ surrounds, like a cap, the dentine germ on all sides, with the exception of the base. From the dentine germ, however, the cement germ is everywhere separated—first, by the *membrana intermedia*, and second, by the enamel cells of the crown.

In the earliest period the cement germ is a limpid or slightly reddish fluid. The only solid bodies which occur in this substance are the primordial cells. After the cement germ has attained a slightly gelatinous consistence, the cell-membrane of the primordial cells shoots out on all sides numerous prolongations, so that the cells become branched or stellate. From the prolongations are given off finer branches. At last, the branches of several cells meet each other and inosculate.

The cell membrane, which, as well as the ramifications, is very pale and delicate, is expended in the formation of the increasing number of prolongations; so that the appearance is presented as if the latter proceeded from the nuclei. The nuclei, however, during the changes of the cell, remain unaltered. After the still isolated primordial cells have become stellate, the previously gelatinous germ begins to become more consistent; and a fine, transparent, and structureless intercellular substance is seen, in which the stellate cells are deposited.

The primordial germ now approaches its last stage; the prolongations of the cell membrane have become so long and fine as to be actual fibres. The primordial germ forms a semi-transparent, homogeneous, viscid tissue, in which there are bundles of fibres, very fine and smooth. The fibres run parallel, seldom irregularly or intercrossing, and do not ramify. On the individual fibres are long, fine, fusiform nuclei, evidently the remains of those nuclei found in the primordial cells, which were at first round, and afterwards stellate and ramified.

Hereupon closes the primordial stage of the cement germ. From being a fluid, limpid mass, the germ acquires a gelatinous consistence and colour, and is at last changed into a fibrous cartilage: for in the fibrous substance at last formed in the primordial germ there appear cartilage cells. Hannover has observed distinct cartilage cells in the back teeth of new-born infants, or infants some months old; but in human teeth the further development of the cement germ, at least around the crown of the tooth, is arrested, and the fibro-cartilage is not converted into true cartilage and bone, as in animals. For the study, therefore, of the second stage, which comprises the appearance of cartilage cells, Dr. Hannover takes the tooth of the new-born foal.

The cartilage cells appear isolated in the fibrous primordial mass, and without connexion with it. They are of different sizes, round or slightly oval, have coarsely-granulous contents—or rather, coarsely-granular nucleus—but no distinct nucleolus. As they increase in number, which they do first towards the *membrana intermedia*, the fibrous structure of the substance disappears, and it becomes more uniform.

Endogenous development, the presence of two cells in one mother-cell, or of several nuclei in the same cell, Dr. Hannover has not observed. Dr. Hannover remarks, in a note, that what are usually called cartilage cells, he does not consider cells, but only nuclei. Complete cartilage cells, he says, occur rarely; most frequently they are met with in enchondroma. Nevertheless, to avoid confusion, Dr. Hannover continues to use the name, *cartilaginous cells*.

The transition of fibro-cartilage into *bone*, in the third stage of development of the cement, commences soon after the appearance of the cartilage cells.

The ossification of the cement begins with an earthy deposition in the intercellular substance. This takes place partly in the form of a coarsely-granular crumbly mass, partly in the form of shorter or longer fine fragments, arranged in definite directions. The fragments are, however, perhaps only fragments of branches of bone-corpuscles which lie in another plane, and have been cut through in making the section for examination.

As the earthy deposition increases, the cartilage cells (nuclei) are pressed toge-

ther, and become small and angular. Pointed prolongations shoot from them, being the first beginnings of the future branches of the bone-corpuscles.

The bone-corpuscle is not a cavity in which can be distinguished a surrounding membrane and proper contents; it is, on the contrary, a solid body arising from the conversion or metamorphosis of the gelatinous firm substance of the cartilage cell.

The Haversian canals make their appearance in the primordial germ as soon as the formation of the cartilage cells begins, or perhaps somewhat later.

The walls of the Haversian canals ossify at the same time with the ossification of the rest of the intercellular substance.

3. *Enamel Germ.*

The investigation of the development of the enamel is also most conveniently made on the teeth of new-born infants.

When the dental sac is opened under water, without tearing the *membrana intermedia*, the cement germ escapes like a fine cloud in the water; but if the *membrana intermedia* is at the same time opened, the cloud is mixed with the enamel cells, or earliest elements of the enamel. These cells are usually united into a sort of membrane.

The enamel germ consists throughout of cells. There is no intercellular substance. In its earliest stage—e.g., in the first permanent back-tooth of the new-born infant—the cells are found still isolated.

The isolated enamel cells are usually round, seldom oval, somewhat smaller than the primordial cells of the cement germ, but much more coarsely granular and darker. They also appear softer. The nucleus is small, round, oval, or angular; has a sharp linear contour, and a peculiar glance. One or two punctiform nucleoli are commonly visible. Two cells enclosed in a mother cell are sometimes seen, and the occurrence of two nuclei in the same cell is likewise not uncommon.

The first change which the enamel cells undergo, after being at first isolated and round, consists in their becoming closely aggregated together, and assuming an angular form, so that, in their membraniform union, they present almost the appearance of a tessellated epithelium. The cells lie pressed together, but each individual cell maintains its independence without coalescing with its neighbours, so that their boundary contour-line is always visible.

The membraniform expansion of the enamel cells is at first very soft and thin, but afterwards becomes firmer, so that it may be separated under water, in small flakes, from the *membrana intermedia*, or from the dentine germ.

The cells, on account of the complete want of intercellular substance, being continually pressed against each other, their angular form is changed into that of longish rectangula, the ends of which are abruptly cut off or rounded. One end is thicker than the other, or one end is pointed—a disposition which becomes more marked in a subsequent stage. In the middle of the rectangulum is the nucleus.

These elongated rectangular cells are, for the most part, disposed vertically between the *membrana intermedia* and the dentine germ. They adhere more firmly to the *membrana intermedia;* and to the end corresponding to this membrane, their nucleus is nearer than to the end next the dentine germ.

The cells are continually growing. The end fixed to the dentine germ increases in length, whilst the nucleus end always remains nearest the *membrana intermedia.*

Even at an early period of their development, the enamel cells are, at the end next the dentine germ, pointed, or even drawn out into a filament; or, as the late Mr. Nasmyth described it, "furnished at one extremity with a delicate prolongation."

The origin of this filament is unknown to Dr. Hannover. It usually appears as a continuation of the cell, and the longer the filament, the more slender the cell itself; but, on the other hand, it is to be observed that the contour of the filament is sharp, and does not appear to be formed of the cell-membrane, the contour of which is soft-looking.

After the cells have attained a certain length by continued growth of the end next the dentine germ, their development passes into the last stage, that of *calcification*. The deposition of earthy matter into the cells takes place first in the ends next the dentine, and then spreads in the direction towards the *membrana intermedia*. Even when one part of the cell is calcified, the remaining part is still soft, and always recognisable by the nucleus at the end next the *membrana intermedia*. The nucleated end is the last to become calcified.

As there is every reason to admit that the same individual enamel cell extends from the dentine germ to the *membrana intermedia*, it follows that the enamel cells are the longest cells in the human body, their length corresponding to the thickness of the completely-formed enamel.

As the enamel cells become calcified they assume the form of six-sided columns, and most probably they assume this form even some time before calcification.

It has been above stated that the enamel cells are disposed vertically, but this is not exactly the case always, for there are parts of the enamel in which the cells form a more or less sharp angle with the *membrana intermedia* and the dentine germ. Moreover, the columns are not always straight, but very often undulating, and sometimes they even cross each other.

4. *Membrana Intermedia.*

This is a fine thin membrane, which must not be confounded with the membraniform expansion of the enamel cells, and which lies on the inside of the cement germ, between this and the enamel cells.

It consists of a homogeneous substance, in which numerous small round or oval, angular or pointed nuclei, without distinct nucleoli, are embedded. The boundary towards the cement germ is sharp and linear, and the cells of the cement germ lie compressed on it. The boundary towards the enamel cells which sit on the opposite surface, is likewise sharp. The enamel cells admit of being easily detached from the inner surface of the *membrana intermedia*; whilst, on the contrary, it is not without difficulty that the *membrana intermedia* can be separated from the cement germ.

The *membrana intermedia* does not belong exclusively to the crown or the enamel, but is continued on the root, where it separates the dentine from the cement; thus lying, as in the crown, on the inside of the cement. Dr. Hannover has, however, not been able to isolate it here, because, immediately on the formation of the outermost stratum of dentine, it forms an adhesion with it, and can only be recognised in the fully-formed tooth as the *stratum intermedium*.

General Remarks on the Order of Succession in the Development of the Dental Elements in the Mammifera.

According to the view, that the development of the teeth begins with the formation of papillæ at the bottom of primitive grooves in the jaws—grooves at first open, but which afterwards close and become subdivided by transverse partitions—according to this view, the papilla is the future dentine germ; the cement germ is formed in the follicular stage of the development, after that the previously open papilla-cavity has closed, by deposition of a granular substance. The formation of the cement germ takes place, therefore, subsequently to that of the dentine germ.

If we view the enamel cells as a continuation of the epithelium of the mucous membrane of the mouth and of the dental groove, the foundation of the enamel must exist before the formation of the cement germ; and this would argue that the formation of the enamel is independent of the cement germ or the previously so-called enamel organ.

Although, however, the order of succession in the development of these three dental substances appears to be as stated, it is not a uniformly progressive one, and there are periods when the development of the one substance appears to anticipate that of the other.

The dentine germ is found at first as a solid body at the bottom of the dental groove and of the dental sac. The point or free border appears earliest, is composed of cells at first isolated, but which subsequently elongate, meet each other, and coalesce into long rows; lastly, the dentification of the tubes commences also at the point, and this takes place before any one of the other substances has attained its permanent form.

The resemblance of the enamel cells to a tessellated epithelium is unmistakeable, and in so far as the epithelium of the dental groove exists before the papilla, it may be asserted that the enamel is the first formed substance of the tooth. Nevertheless, the cells in question first acquire the character of enamel cells only after the papilla has sprung forth. It is here also the point of the papilla on which the enamel cells, after having acquired the elongated form, first arrange themselves beside each other, and become united into a fine membrane, the peripheral part of which is always found in a less advanced stage. Likewise, the calcification of the individual cells commences first on the point or the free border of the dentine germ. The end towards the dentine germ calcifies first, is supported only on the dentified dentine germ; whilst the root end of the cell, which contains the nucleus, is in close union with the *membrana intermedia*. With the calcification of this end the formation of the whole enamel column is completed.

Dr. Hannover has not yet satisfactorily traced the first formation of the *membrana intermedia.*

The cement germ arises from the primordial germ, which is at first fluid, subsequently gelatinous, then converted into fibro-cartilage, and lastly ossified. In its development, it is always behind the other dental substances. The ossification commences first in the part lying next the *membrana intermedia*, but the ossification can only commence at the period when the enamel cells are completely calcified, and the enamel has acquired its permanent thickness. The slower and later growth of the cement germ is easily observed.

In man, the crowns of the teeth are covered by a cement germ, but it never ossifies there. In ruminants—the elephant, &c.—the crowns are covered with a strong development of cement, both around the circumference generally, and down between the pointed parts. The difference is owing to this,—that in man, the crown, by its growth, pushes through the cement germ in cutting the gum, and a stop is put to the further development of cement. It is thus also that we find, in the ruminants and horse, in teeth which have just cut the gum, large openings in the middle of the cement, which are owing to the arrested ossification of the cement germ, occasioned by the protrusion of the crown through the gum.

When the formation of the enamel is completed, and the root commences to form, the dentine germ is very closely surrounded by a firm membrane, which consists of the condensed remains of the *membrana intermedia* and cement germ, and contains small nuclei and fibres closely pressed together, the latter belonging to the cement germ. Cartilage corpuscles Dr. Hannover has not observed in it—at least, not in the upper part of the root of the human incisor.

All the dental substances increase in hardness during their growth, which is probably occasioned partly by the loss in water, partly by the stronger pressing together of the elementary parts.

Part II.—Histology of the Teeth of the Mammifera.

1. *Dentine.*

The dentine forms the largest portion of the tooth, is non-vascular, white, semi-transparent, and intermediate in hardness between the cement and the enamel. It is composed of thick-walled tubes, imbedded in an intertubular substance.

The walls of the dentine tubes are very thick in proportion to their lumen; they cannot, however, always be distinguished, because they usually coalesce with

the intertubular substance. The thickness varies in different animals; in the horse they are very thick; thinner in oxen and in man. The greater apparent fineness of the tubes towards the periphery of the tooth, depends on the narrowness of the lumen, not on a greater thinness of the walls of the tubes themselves.

The contents of the tubes are in the fresh state composed of a transparent fluid, with calcareous matter in solution; in dry teeth—such as are commonly made use of for making sections—the tubes appear empty, or filled with earthy particles. When the tube is empty, the lumen is seen, even in fine tubes, bounded on either side by a dark line. The lumen itself is clear, and the clearness increases on the addition of a fluid, without, however, being accompanied by a disengagement of air globules. The same tube may be at one place empty, at another filled with earthy particles.

A circulation of nutritive fluid in the perfectly-formed dental tubes, such as has been supposed by Krukenberg, cannot, Dr. Hannover says, be admitted.

The peripheral end of the dentine tubes calls for some detailed consideration. According to Dr. Hannover's observations, the outermost ends of the trunks and branches are lost in the surrounding intertubular substance, their walls coalescing with it. Loops, or anastomoses with neighbouring or more distant trunks and branches, are only exceptionally formed. Many branches, however, sink into small calcareous cavities.

The admission of an anastomosis between tubes is often founded on the appearances presented in those cases in which the original globular basis of the dentine has remained still visible, the interspaces and passages between the globules in such a case being mistaken for branches of dental tubes.

Contrary to a common opinion, Dr. Hannover insists that there is nowhere any connexion between the dentine tubes and the ramifications of the bone corpuscles of the cement, the dentine and cement being separated from each other by the *stratum intermedium*.

Dr. Hannover can as little admit a communication of dentine tubes with the enamel. The idea of a transition of dentine tubes into tubes between the enamel fibres, is completely opposed by the development of these two substances. The appearances which have been supposed to indicate a transition, Dr. Hannover thinks must have been in some cases owing to the sections having been oblique, so that a part of the dentine overlapped the edge of the enamel; in other cases, the passages or striæ into which the dentine tubes appeared to pass may have been merely an optical expression of the angular form of the enamel columns.

Earthy deposits, sometimes met with in the substance of the dentine, having some resemblance to bone corpuscles, have been mistaken for such, and the erroneous notion of a transition of dentine tubes into bone corpuscles founded thereon.

The intertubular substance in which the dentine tubes are embedded is, as is known, clear, homogeneous, and structureless.

The substance in which the lumina of the dentine tubes are embedded, also appears in a form which recals to mind the development of the dentine tubes from round cells, and most probably does depend on this original form.

This is a point not yet completely cleared up; Dr. Hannover therefore expresses himself cautiously in regard to it, and speaks of the substance only as it has appeared to him in many cases between the lumina of the dentine tubes. In man, Dr. Hannover has often observed it, but not nearly so distinctly as in the ox, the horse, and most cetaceans. In the ox, for example, Dr. Hannover found, near the enamel, a globular structure in which the individual globules were separated by very clear passages from each other. By the anastomosis of the passages, there was produced a retiform appearance, which has erroneously given origin to the admission of a retiform anastomosis between the ramifications of the dentine tubes: the proper ramifications ran past these passages without forming any junction with them. The globular structure consisted of round or oval bodies, of the size of a

human blood-corpuscle to that of a frog's; sometimes, when the bodies were pressed against each other, they were angular. Their substance was somewhat more granular and less transparent than the rest of the dentine. On and between the globules were shapeless earthy masses, similar to those above noticed. They lay partly close to the boundary of the enamel, partly at some distance from it, and had a deceptive resemblance to bone corpuscles, for which they have been mistaken by those observers who admit a transition of the ramifications of the dentine tubes into bone corpuscles.

The innermost part of the tooth, or that part of the germ which is latest of dentifying, presents in most animals indications of imperfect development. Very generally it consists of an irregular aggregation of imperfectly-formed dentine tubes. This is the case in almost all teeth, and the difference from the rest of the dentine is perceptible even to the naked eye, on account of the greater transparency of the parts. The lumina of the tubes are seen in a structureless, clear mass, frequently in branches irregularly winding or suddenly interrupted, in fragments of different length and form, diverging in their direction, however, usually from within outwards. Their thickness does not appear, on the whole, changed; the branches, on the contrary, which are sometimes absent, are in other cases very numerous, and finer than elsewhere. Dr. Hannover thus found it in man, the dog, the bear, sow, horse, &c., in which the fragments frequently form large loops. Often the inner transparent part, in which these irregular tubes occur, is sharply defined from the rest of the dentine, and forms a centre of greater or less extent, outside which the regularly-arranged dentine tubes begin; for example, in the sow, horse, ox, &c. In the otherwise uniform clear mass in the axis of the tooth, the globular form of the intertubular substance may also, in rarer cases, be recognised, as in the dugong. In other instances, there is a total absence of dentine tubes in the innermost part of the dentine, and the dentified germ consists in the middle of only a uniform clear mass.

True Haversian canals of the same nature as in the cement do not occur in the dentine. There are, however, in the middle of the dentine, canals which much resemble the Haversian canals, but which have probably a different origin. They are met, for example, in the ox, in the form of round or oval, though more frequently irregular, sections, filled with a yellow granular mass, or they appear black and opaque when extraneous matter has penetrated them in grinding. Dr. Hannover believes them to be remains of vessels. The canals mentioned by Owen and Tomes, in the inner part of the dentine of several rodents, should not be looked upon, Dr. Hannover thinks, as Haversian canals, especially as, according to Tomes, they run parallel with the dentine tubes, and do not anastomose, but branches of the dentine tubes open into them. It is therefore improper to speak of the vascularity of dentine in the same sense as the vascularity of bone is spoken of. The admission of Haversian canals in the dentine is founded only on the incorrect comparison of the dentine with bone, and the process of dentification with that of ossification; therefore, also, the name of *osteo-dentine*, which Owen has given to the central clear substance, with free and irregularly-disposed dentine tubes, in several mammifera, is objectionable.

As in the interior of the dentine, canals occur which have been taken for Haversian canals; so there also occur in it forms which are very similar to bone corpuscles, and which have been mistaken for them. They occur in the ox and bear, but are most frequent in cetaceans. Dr. Hannover believes them to be of the same nature as the pulp and pulp-cavity.

A similar origin, though a somewhat different structure, most probably belongs to the various forms which occur in the so-called granular substance (*Poudingue*, Cuvier). This substance is of very general occurrence in the interior of the teeth of the sea mammifera; in other animals it is only exceptionally present. In no respect, Dr. Hannover says, can the structure of the granular substance be compared, as Retzius thinks it may, with the structure of the cylindrical bones. Both the anatomical and histological structure of the substance in question, Dr. Hannover insists, is opposed to any resemblance with bone.

When the tooth does not remain open throughout the whole period of life, the point of the root is gradually closed by the external cement; the growth of the dentine is ended, and bloodvessels and nerves can no longer penetrate the pulp-cavity. In some teeth, a cavity is still found filled with the remains of the dentine germ; but in most cases the whole germ is dentified. The consequence of arrested nourishment is especially evident in the point of the root. Not only with the microscope, but also with the naked eye, it is frequently seen that the dentine and the cement are not sharply separated, as in the other parts of the tooth; but that the transition between the two takes place in an unmarked manner. Therefore, the irregularly-disposed dentine tubes mingle with bone corpuscles, without, however, this exceptional simultaneous presence being anything more than juxtaposition or accidental mingling. The ramifications of the dentine tubes, which are here often in extraordinary number, do not pass into the ramifications of the bone corpuscles. Sometimes the dentine tubes, sometimes the bone corpuscles, have the predominance. When the cement has been formed in great masses around the point of the root, the Haversian canals characteristic of this substance are also met with. The granular substance, likewise, together with its irregular earthy deposits, may be found mingled in the point of the root with the cement; and here the best opportunity is presented of observing the distinction between those earthy deposits and the bone corpuscles. Lastly, irregular smaller and larger cavities are met with, which are owing partly to an incomplete dentification, partly to the remains of bloodvessels, but which are not to be mistaken for Haversian canals.

2. *Cement.*

In the history of the development of the tooth, it has been shown that each *dentine germ* is originally surrounded by a *cement germ*, and is separated from it in the crown by the *enamel germ* and the *membrana intermedia*, but in the root by the *membrana intermedia* only. When the different dental substances have acquired their permanent form, the cement can therefore never be in immediate contact with the dentine; it is only at the point of the root, as has just been shown, that a mingling of the two substances takes place. Towards the enamel the cement is distinctly limited; it never mingles with the enamel columns, and easily separates from them completely, whilst the connexion with the dentine in the root is very firm.

Cement exists around every root, but not around every crown; for in teeth with conical dentine germs, the cement germ in general aborts round the crown, and does not ossify. The quantity of cement formed around the root of the conical dentine germ is very variable. Whilst the cement around the root of the teeth of man and the dog forms only a thin layer, its quantity in the dolphin is very considerable, and in the physiter almost as great as that of the dentine; as in these animals the crown of the tooth is very small, and soon worn away, it may with truth be said of them that they masticate with the roots of the teeth. In teeth with notched dentine germ, the cement, which is considerable in quantity, first surrounds the enamel of the crown, and then the dentine of the root. In teeth with a cup-shaped dentine germ, the outer cement exists only in small quantity, the inner predominates, and is distinguished, as before mentioned, from the outer by its yellow colour and opacity, the greater number and the size of the less-branched bone corpuscles, and the wide-spread Haversian canals. The inner cement, in particular, resembles still more closely true bony substance than the outer. In teeth with foliated dentine germs, the quantity of cement between the leaflets is considerable; in the periphery, small. Although the cement is not everywhere of the same thickness, still its quantity on the whole increases in all teeth from above downwards.

The cement, which is opaque, is softer than dentine, and is therefore always most worn away on the masticating surface; on the contrary, it is harder than bone. Its colour is dull white, grey, or yellowish, and at the same time speckled. In other respects the structure of the cement in general is similar to that of

osseous substance, and both are distinguished by bone corpuscles imbedded in a ground substance, and by Haversian canals; the bone corpuscles in bone are, however, usually more numerous, larger, and darker; the arrangement and number of the Haversian canals and the ground substance are also different.

As the cement does not lie in immediate contact with the dentine, there can be no communication of the ramifications of the bone corpuscles of the former with the ramifications of the tubes of the latter; in the dentine itself there are no bone corpuscles, as has been above insisted on.

Although the form of the bone corpuscles of the cement is very irregular, still their direction is, in general, such that their longest diameter is perpendicular to the length of the tooth. Around the Haversian canals the bone corpuscles lie, sometimes in no definite order, sometimes in concentric series, as in the bones; this is the case, for example, in the inner cement of the ox.

The number of bone corpuscles corresponds to the quantity of cement. They are entirely absent where the cement is thin, as in the neighbourhood of the crown of teeth with conical dentine germs—e.g., in man, dog, trichechus; in man, the part of the cement next the crown is quite clear and very brittle, so that fissures occur in it, as in the enamel.

The ground substance, in which the bone corpuscles are imbedded, is, in most cases, clear and transparent, but not so much so as the dentine. In other cases it is granular. In many it has the appearance as if stratified.

The Haversian canals of the cement do not completely agree with those of bone, in respect to their structure, number, and arrangement. They appear to have walls distinct from the rest of the cement. They serve for the passage of bloodvessels, which penetrate from without inwards. Where the cement forms only a thin layer, they are wholly wanting in all animals. In man, Haversian canals are met with only at the point of the root where the cement is in great quantity. In the horse and ruminants the cement, especially the inner, is pervaded by numerous and large Haversian canals. The opening of the ramifications of the bone corpuscles into the canals, Dr. Hannover has not been able to observe, such as is stated to be the case by Owen in the megatherium, in which the canals at the same time anastomose with each other.

The irregular cavities often seen in the point of the root above-mentioned in the description of the dentine, must not be confounded with Haversian canals. These cavities, first observed by Czermak in human teeth, are branched, thick, varicose canals which pervade the cement in different directions, penetrate it from without inwards, and reach as far as the dentine by their blind end. They appear to be abnormal.

Different also from Haversian canals are fine passages which occur even in thin layers of cement, and have a distant resemblance to dentine tubes, but which are seldom or never branched, and usually run transversely. The nature of these passages is still unknown. According to Tomes and Kölliker, they are frequently connected with the dentine tubes and bone-corpuscles. That they are connected with the dentine tubes, Dr. Hannover most decidedly denies, because the dentine is everywhere separated from the cement by the granular *stratum intermedium*. As to their connexion with the bone corpuscles, Dr. Hannover has observed this only in the dugong; in all other animals he has expressly remarked that they have no communication with the bone corpuscles.

3. *Enamel.*

The enamel characterizes the crown of the tooth. It is always interposed between the dentine and the *stratum intermedium*, outside which is the cement; where, however, the cement does not ossify, the enamel is covered only by the *membrana intermedia*, which then appears as the so-called enamel cuticle. This is the case in teeth with conical dentine germs; and Owen has probably confounded the *membrana intermedia* with the cement, when he speaks of the occurrence of a very thin layer of cement on the crown of the tooth in man and the ape. The enamel

cuticle is soon worn away after the crown has broken through the gum, so that the enamel is laid bare. The enamel is in general thickest above, especially on the masticating surface; below, it is thinner, and ends with a free border, which is frequently covered with a thin layer of cement.

Although it cannot be said, with Owen, that the enamel is the least constant substance of the tooth, because every tooth possesses a crown, still Dr. Hannover remarks that the normal relations are frequently changed at an early period by the wearing away of the crown. Thus, very commonly the teeth of trichechus, dolphins, edentata, the incisors of the elephant, &c., are found without enamel—i.e., the crown, being so small, is soon worn away, and only the root remains; in the teeth of the young animals, however, the enamel of the crown is distinct enough.

The enamel is composed of columns developed from the enamel cells. The typical form of these columns appears to be six-sided. This form is, however, usually changed by the mutual pressure of the columns into a polygonal or flattened one. The thickness of the columns is variable. The columns present more or less distinct transverse markings, having some resemblance to those of muscular fibres. The cause of these markings lies in the calcification of each individual cell taking place in strata, hence the appearance is often more distinct in young than in old animals.

The direction of the enamel columns is either perfectly straight, or bent, or slightly tortuous. In regard to this, however, there is no definite rule, for in the corresponding tooth of the same order of animals, or of a different order, sometimes one, sometimes another direction of the enamel columns is observed. In the human tooth, for example, the columns are found sometimes straight, sometimes bent, sometimes wavy or intercrossing. As to the cause of this difference: the origin of the straight or wavy course in one direction it is easy to explain, but not so the crossing of straight or spiral columns.

Between the enamel and dentine there is no special membrane. The enamel columns directly touch the dentine. Dr. Hannover also denies that there are any such depressions on the surface of the dentine for receiving the ends of the enamel columns, as have been supposed by Lessing and Owen. In all the teeth of different animals examined by Dr. Hannover, he found the limit between the enamel and the dentine always sharply defined, and formed of a single dark line. Owen mentions a layer of cells between the dentine and enamel, in regard to which Dr. Hannover remarks that it is not quite clear to him what could have deceived that observer into this notion, unless it was the globular formation of the intertubular substance of the dentine, which in the ox, for example, may, like small protuberances, project into the enamel.

4. *Stratum Intermedium.*

This forms in the crown the limit between the enamel and the cement; and in the root, the limit between the dentine and cement. It is the metamorphosed *membrana intermedia* which, during the development of the tooth, is closely connected by its outer surface with the cement germ, and on the inner has attached to it the nucleated ends of the enamel cells.

The structure in question attains its permanent form after the enamel cells have become completely calcified in their entire length; as it is situated between the enamel cells and the cement, the ossification of the cartilaginous cement germ can commence only after the complete development of the *membrana intermedia.* It, therefore, always separates, in the crown, the enamel,—in the root, the dentine, from the cement. But as the cement germ round the crown of teeth with conical dentine germs does not in general ossify, but becomes aborted, so the *membrana intermedia* lies free on the surface of such crowns, and forms on the as yet unworn teeth the so-called enamel cuticle above noticed. This name, which was given to it by Kölliker, Dr. Hannover observes, the membrane merits neither by its origin, nor its structure, nor its nature. For the *membrana intermedia* does not belong

exclusively to the enamel, nor does it lie superficially like a cuticle; whilst its structure, as before shown, has nothing in common with that of epidermis.

Erdl first demonstrated this membrane by means of the application of diluted hydrochloric acid; according to him, it has an epithelium-like aspect, and appears to be composed of small cells. By Owen it was probably mistaken for a thin layer of cement. Nasmyth supposed that he was able to trace it not only on the outer surface of the enamel of the human tooth, but also on the outer surface of the root; he called it "the persistent dental capsule," a name, Dr. Hannover observes, which might well be adopted, as the *membrana intermedia* does indeed represent a sac-like structure in the dental sac, and can be also still demonstrated in the fully-formed tooth. As, however, the membrane, though it covers the surface of the enamel, does not cover the outer surface of the root, as Nasmyth supposed, but is situated between the cement and the dentine of the root, Dr. Hannover does not consider it proper to retain the name of "persistent dental capsule," but proposes instead that of *stratum intermedium.*

What becomes of the *membrana intermedia* in the crown between the enamel and the cement, Dr. Hannover has not clearly made out. The limit between those two substances is formed of a dark, well-defined, but irregular line, sometimes double. There is no interspace between the enamel and the cement, only where they have split from each other—which readily takes place in drying or in making the preparation—there is formed an empty space, which is often filled with foreign matter. It thus appears that the *membrana intermedia* in the crown, which is so evident in the early stage, is in the completely-formed tooth no longer to be observed

It is otherwise with the *membrana intermedia* in the root between the dentine and cement. Here it is converted into a particular stratum, having the aspect of a clear line of variable breadth, but always narrower than the *membrana intermedia,* whence the *stratum intermedium* has arisen, and opposes any communication of the dentine tubes with the ramifications of the bone corpuscles of the cement.

The stratum usually presents itself as a clear streak running along the whole length of the root; a frequent variation in its aspect, however, is occasioned by the deposition in it of masses of finely or coarsely granular earthy matter. These masses are usually collected towards the dentine in greater quantity, whilst the limit towards the cement is more defined. At the same time, the commencement of the dentine tubes is concealed among them. Such an appearance has especially occasioned the already often-mentioned erroneous supposition that dentine tubes pass into bone-corpuscles to which those masses very often have a great resemblance. The perfectly clear or the granular and dark aspect of the *stratum intermedium* may be observed in the same tooth; much depends on the thickness of the preparation, and it is especially in thick preparations that the shapeless, earthy masses of the *stratum intermedium* are most readily mistaken for bone corpuscles. This, according to Dr. Hannover's experience, was the case with Retzius's preparations of the teeth.

From Dr. Hannover's observations, it is seen that the *stratum intermedium* may present itself with a very variable aspect, but it always serves for the separation of the dentine from the cement. Only in the point of the root it is frequently indistinct, and there arises that previously-mentioned mingling of dentine and cement, but which is not accompanied by a real communication of the dentine tubes and the ramifications of the bone corpuscles.

The *stratum intermedium* has been seen by some previous observers, but its nature not recognised, because the *membrana intermedia,* whence the stratum arises, had been overlooked.

HALF-YEARLY REPORT ON PHYSIOLOGY.

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I. Food and Digestion.

1. F. S. Smith: *Experiments on Digestion.* (The Medical Examiner, vol. xii., July and September, 1856. Philadelphia.)
2. Hanlo: *De Extracti Carnis Frigide Parati, virtute nutrienti.* (Trajecti ad Rhenum, 1855, and Schmidt's Jahrb., vol. xci. p. 145, 1856.)
3. Jos. Jones: *Digestion of Albumen and Flesh, and the Comparative Anatomy and Physiology of the Pancreas.* (The Medical Examiner, vol. xii., May, 1856. Philadelphia.)
4. Skrzeczka: *Quæritur, quomodo Caseinum et Natrum Albuminatum pepsino afficiantur.* (Diss. Inaug., Regiomonti, 1855, and Canstatt's Jahresber. d. Physiologie für 1855, p. 184.)
5. F. Hoppe: *On the Influence of Cane Sugar on Digestion and Nutrition.* (Virchow's Archiv, vol. x. pp. 144 ss., 1856.)
6. Colin: *On the Formation of Sugar in the Intestinal Canal, and its Absorption by the Chyliferous Vessels.* (L'Union, No. 41, 1856; and Schmidt's Jahresb., vol. xc. p. 273.)
7. Funke: *Contribution to the Physiology of Digestion.* (Siebold and Kölliker's Zeits. für Zool., vol. vi. p. 304, and vol. vii. p. 315.)
8. Zenker: *On the Chyliferous Vessels in the Intestinal Mucous Membrane.* (Zeits. für Zool., vol. vi. p. 321.)
9. C. E. E. Hoffmann: *On the Absorption of Mercury and Fat, &c.* (Würzburg, 1854; and Canstatt, l. c. p. 80.)

Alexis St. Martin, the Canadian, so well known through Beaumont's experiments, has again been made the subject of scientific observation by Dr. Smith, of the Pennsylvania College. This author occupies himself, in the present essay, principally "with the nature of the acid contained in the gastric juice, and the influence of this secretion upon the various alimentary principles as classified by Prout, to wit, saccharine, oleaginous, and albuminous food." Regarding the temperature of the stomach, Smith observes, that it was during the progress of digestion about 100° to 101° F.; in the state of fasting, 98° to 99°. The reaction of the gastric fluid, while digestion was going on, was invariably, and with all kinds of food, acid; while the fluid obtained from the empty stomach was always neutral. Concerning the cause of this acidity, repeated experiments and analyses (the latter performed by Professor R. E. Rogers, of the Pennsylvania University) lead to the following conclusions:—1. That the acid reaction is not due to the presence of *phosphoric acid*, or acid phosphate of lime:* 2. That if *hydrochloric acid* is present, it is in very small quantity: 3. That the main agent in producing the acid reaction is *lactic acid*.

In regard to the influence of the gastric juice upon *albuminous* substances, the author offers the following experiment:—"Four ounces of rarely-done beef-steak were given to St. Martin at 10 A.M., May 5th, after a light breakfast of bread and coffee, at 6 A.M., of the same day. No fluid was allowed to be taken in connexion with the beef, nor any other article of food. At 12 M., of the same day, St. Martin was subjected to examination. On pushing back the fold of mucous membrane which acts as a valve to the fistulous orifice, a considerable amount of fluid was readily distinguishable in the stomach, mixed with bubbles of air, but no solid matter was visible. About a fluid ounce and a-half of this fluid was withdrawn from the stomach by a catheter, with the effect of producing nausea, which pre-

* Blondlot: Traité Analytique de la Digestion. 1843.

cluded the possibility of obtaining more. . . . . Specific gravity, 1·009. Numerous flocculi were visible to the naked eye. . . . . It was almost entirely inodorous, viscid, and to the taste decidedly acid. The microscope revealed amorphous granular matter, mucous corpuscles, granular cells, and a few epithelial cells; a few transversely-striated muscular fibres, some almost uninjured, some broken down, and with the sarcous elements liberated. Numerous oil-globules were also distinctly visible, and a few fibres of yellow elastic tissue. The bulk of the material consumed as food had undergone entire solution, and had wholly lost its characteristic appearance. A portion of the supernatant fluid was boiled actively, without, however, presenting the slightest trace of coagulation. The mineral acids had no effect upon it while cold, but when boiled with strong hydrochloric acid, the purple colour of the protein bodies was distinctly manifested. The addition of acetic acid rendered the fluid rather more clear than before. The action of alkalies on the fluid was not tried; but Trommer's test gave no evidence of the presence of glucose." (*loc. cit.* p. 515.)

It is easy to recognise in this description the characters of Mialhe's *albuminose*, or Lehmann's *peptone*.

"The conclusion from this observation is, that *the gastric juice is a true solvent for animal food.*" (*loc. cit.* p. 515.)

From the number of oil globules visible under the microscope, the author is inclined to support the view "that *fatty matters* undergo no change in the stomach beyond that of disaggregation."

In order to show the influence of gastric juice on the *amylaceous* articles of food, Smith relates two experiments. In one of them a portion of wheaten bread was given to St. Martin while fasting, which was deliberately masticated by him; in the other, a portion of bread moistened with water, was introduced into the fistulous orifice, "and St. Martin was requested to swallow as little saliva as possible, which, as he used tobacco, he had little difficulty in complying with." After two hours and a half in the former, and after an hour and a half in the latter experiment, the contents of the stomach were withdrawn and submitted to examination: the result of which lead the author to the following inferences:— "That starchy materials are digested in the human stomach; that human gastric juice does not prevent the conversion of starch into grape-sugar, and that this conversion may take place in the stomach independently of the action of saliva." (*loc. cit.* p. 518.)

We have purposely apportioned more space to the description of these three experiments, on which the author's conclusions regarding the action of the gastric juice are based, and have given these conclusions in his own words, in order that the reader may be better enabled to judge for himself of the value of these observations on so important a subject.

Hanlo made on himself experiments with the extract of meat prepared according to Liebig's prescription (i.e., without heat); with broth (bouillon) prepared from the same quantity of meat; with roasted meat itself; and with the "decoctum album" (a decoction of scraped hartshorn, bread, and sugar): the food taken besides these substances, and the general mode of living, remaining the same during all the experiments. By comparing the quantity of urea and chlorides excreted through the kidneys during the experiments with the four above-mentioned articles of diet, Hanlo comes to the inference, that the body assimilates more nutrient material from the extract of meat than from the "bouillon," and much more than from the "decoctum album." The quantity of urea excreted during the consumption of the extract, exceeded even that obtained during the experiment with the roasted meat! But whether this is a result constantly met with also in other individuals, and whether it justifies the conclusion, that we derive more nutritious matter from the extract than from the meat in substance, we must as yet consider as undecided. The albumen and hematosin are the principal substances to which the extract owes the preference over the "bouillon."

In opposition to Lehmann, Bidder and Schmidt, and other physiologists, Jones maintains the view, "that meat is entirely digested in the stomach." This view

is based on the fact, that the author, in examining the contents of the stomachs of fishes, reptiles, birds, and mammalia, in every stage of the digestive process, never has discovered undigested particles of flesh in the small intestines (p. 259). He has convinced himself "that, in the normal process of digestion, the matters dissolved by the gastric juice are almost immediately absorbed or pass into the duodenum." Jones considers it therefore "evident that this process is far more energetic than that of artificial digestion," and that therefore "the argument founded upon artificial digestion falls to the ground."

Skrzeczka gives the result of a series of experiments on casein and soda-albuminate under the influence of pepsin (rennet). The coagulation of the milk is considerably accelerated by containing more fat, by the addition of a larger quantity of rennet, by increase of temperature. Boiled milk coagulates later than unboiled. The whey obtained by filtration contains albumen free from alkali, similar in reaction to the white of eggs. Skrzeczka's experiments with alkali-albuminates show, 1, that they are not coagulated by rennet alone; but 2, that coagulation takes place when sugar of milk or butter are added; 3, that this coagulation takes place most quickly when sugar of milk or butter are added in the proportion in which they are contained in the milk; 4, that an artificial milk, prepared of soda-albuminate, sugar of milk, and butter, coagulates after some time spontaneously, quite like genuine milk. Analogous to the soda-albuminate is the reaction of Panum's serum-casein, and of casein obtained by Rochleder's method.

Hoppe's investigations are made principally on a dog, that received daily from 20 to 200 grains of cane-sugar, with or without other food. The author himself draws the following inferences:—1. Cane-sugar was not altered by the saliva and gastric juice within one hour. 2. Larger doses of cane-sugar excited vomiting in one or two hours. 3. Neutralization of the gastric juice, by means of chalk, effected no change in the just-mentioned two observations. 4. When the gastric juice was thus neutralized, yeast did not seem to develop fermentation in the stomach. 5. No trace of sugar was found in the urine or fæces during continued feeding with sugar. 6. The quantity of lactic acid in the urine does not become increased by feeding with sugar. 7. When sugar and meat were given together, the weight of the animal increased much more rapidly than when meat alone was given. 8. When sugar and meat were consumed, urea was excreted in smaller quantity than when meat alone was taken. 9. By exclusive sugar-diet the excretion of urea was depressed to its lowest amount. 10. The excretion of nitrogen with the fæces was not much altered by the addition of sugar to the meat. 11. By the presence of much sugar in the blood, the albuminous substances are preserved from oxidation. The albumen thus stored up appears to become decomposed under the development of fat. In this manner sugar produces fattening only when, at the same time, albuminous substances are liberally supplied. 12. Bernard's* conjecture, that the ingestion of sugar excites only an increased formation of sugar in the liver, while the sugar ingested as such is supposed to be transformed into fat, is untenable; it is likewise unproved that the production of sugar in the liver forms the principal source of animal heat. 13. The temperature of the body did not become increased by the addition of sugar to the allowance of meat. 14. The health of the dog was in no way injured by feeding upon large quantities of cane-sugar in addition to a liberal meat-diet.

Colin found sugar as well in the thoracic duct as also in the larger chyliferous vessels of the mesentery, in animals that had been fed exclusively with animal food. He is thus led to conclude, that sugar is formed in the intestinal canal, at the expense of the animal constituents of the food.

Funke maintains the view, that there are no actual and constant channels for the passage of fat through the villi, with the exception of a central canal; that the fat enters the epithelial cells of the villi, according to the laws of endosmosis, and finds from thence its way through the parenchyma to the central canal. Funke adds, that frequently several globules, on their way towards the central canal,

* Leçons de Physiologie Expérimentale, &c. 1855.

follow each other closely; that through this succession of globules lines are formed; that the existence of various such lines in different directions produces sometimes the appearance of a network of vessels; but that this appearance is deceptive, as the lines just described do not possess any well-defined membranous walls, and can therefore not be considered as vessels.

Zenker, on the other side, who made his observations on two individuals that had suddenly died soon after meals, holds the view that the network witnessed in the villi is formed by channels with distinct boundaries; that also the mucous membrane between the villi contains many such narrow channels, which lead without interruption into the larger chyliferous vessels.

Hoffmann's researches lead to the inference, that a considerable quantity of fat is absorbed, without any previous change, by the lymphatic and chyliferous vessels. Experiments on endosmosis, made with pieces of small intestines and of the urinary bladder of oxen, show that fat passes through these membranes under a lower degree of pressure, when they have been previously steeped in bile, than without this.

II. Blood; Circulation; Respiration.

1. Bischoff: *Calculation of the Quantity of Blood in a Decapitated Criminal.* (Zeit. f. Wissens. Zool., vol. vii. 3, 1855; and Schmidt's Jahrb., vol. xc. p. 286.)
2. Lehmann: *On the Constitution of the Blood in different Vessels.* (Bericht. d. saechs. Gesells. d. Wissenschaften, Nov. 1855; and Canstatt, l. c., p. 188.)
3. Jos. Jones: *Physical, Chemical, and Physiological Investigations upon the Vital Phenomena, Structure, and Offices of the Solids and Fluids of Animals.* (Hay's American Journal of Medical Science, July, pp. 15 ss. 1856.)
4. Bamberger: *Contribution to the Physiology and Pathology of the Heart.* (Virchow's Archiv, vol. ix. pp. 328 ss. 1856)
5. Chaveau and Faivre: *New Experimental Researches on the Normal Movements and Sounds of the Heart.* (Compt. Rend., Septemb. 1855, pp. 423 ss.; and Canstatt, l. c., p. 82.)
6. Soire: *On the Circulation of Man and Animals.* (Canstatt, l. c., p. 83.)
7. Heidenhain: *Disquisitiones de Nervis, Organisque Centralibus Cordis, &c.* (Berolinae, 1854; and Canstatt, l. c., p. 130.)
8. Poiseuille: *Researches on Respiration, &c.* (Compt. Rend., vol. xli. p. 1072; and Canstatt, l. c., p. 8.)

The researches of Arnsperger, Fano, and Schiff are noticed under 'Nervous System.'

Bischoff calculated the quantity of blood in circulation by means of Welker's method. This consists in taking, first, a sample of the normal blood of the body to be examined, and in washing out afterwards the bloodvessels and the minced organs of the whole body, in order to collect all the blood contained in them. By measuring the whole of the fluid thus procured, and by diluting the sample of the normal blood previously obtained until it has gained exactly the same colour as the fluid of the maceration, one may calculate, through the quantity of water required for this dilution, the quantity of blood contained in the fluid of maceration.

In the case examined by Bischoff, the individual weighed—

	Grammes.
With clothes, before the decapitation	65,780
" after "	62,280
The loss of blood	3,470
Blood contained in the clothes	291
Blood in the fluid of maceration	994
Blood from the hepatic and portal veins	20
Total amount of blood	4,775*

* About nine pounds and a half.

According to this examination, therefore, the weight of the whole of the blood in circulation would be only about $\frac{1}{18}$th of the weight of the body; while Valentin had assumed it to be $\frac{1}{5}$th, E. Weber and Lehmann, $\frac{1}{8}$th. In Weber's case, the loss of blood alone from the decapitation amounted to about 5200 grammes, although the total weight of the body was smaller than in the present instance. For this reason, Bischoff is inclined to doubt the parallelism between the weight of the body and that of the blood, assumed by Valentin. It is however to be observed, that the subject of Bischoff's examination had been affected with symptoms of scurvy for some weeks previous to his death. We may add that, by means of the same method, Welker has found the quantity of blood in animals of different classes, to be about $\frac{1}{13}$th of the weight of the whole body.

Lehmann's researches show that the per-centage of *fibrin* in the blood from the small veins is larger than in that from the arteries; while the blood in the vena cava is poorer in fibrin than that obtained from the arteries. Lehmann therefore considers it probable that fibrin is principally formed in the arteries, that its quantity is increased in the capillaries by the influence of the oxygen, that it perishes again in the larger veins. The per-centage of *salts* is found, by the same author, larger in the arterial blood than in the venous: this circumstance is ascribed to the destruction of organic substances in the lungs, and particularly to that of the extractive matters, but partly also to that of albumen, which is constantly disappearing in a considerable degree; a circumstance manifested by the fact, that the solid residue of the arterial serum contains about 2% less albumen than that obtained from the serum of the venous blood.

The blood from small veins contains about 6% more *water*, and about 6% less dry blood-globules, than that from arteries. The blood from the vena cava (before the inosculation of the hepatic veins) yields, likewise, 2% of globules less, and 2% of water more, than the arterial blood.

The comparison of the blood from the vena portæ with that from the hepatic veins, in dogs and horses, shows that the former contains more serum than the latter; that the serum of the former is richer in albumen, salts, and water than the latter; that therefore a part of these substances seems to remain in the liver. The quantity of extractive matters, on the contrary, is found much increased in the blood from the hepatic veins. The cruor of the latter exhibits a great augmentation of salts, which seems to point to an important alteration, if not new formation, of blood-globules.

Jones's observations relating to the comparative anatomy and physiology of the vertebrate and invertebrate animals, are, to a great part, not new in this country; we give, therefore, only a few of his inferences. Concerning the specific gravity of the *blood*, the author's tables show that, "as the organs, and apparatuses, and intelligence of animals are developed, the blood becomes more concentrated." Regarding the single constituents of this fluid, Jones observes:—The proportion of *water* is greatest in the invertebrata; amongst the vertebrate animals, it is greatest in fishes and aquatic reptiles, smallest in serpents, birds, and mammalia. "It may be laid down as a general law, that, as the organs and apparatuses of the animal economy are developed, and the temperature and intellect correspondingly increased, the blood becomes richer in *organic* constituents." (p. 55.)

In the invertebrata, the number of blood-corpuscles is very small in comparison with the number which exists in the blood of the vertebrata. The increased development of the cerebro-spinal system and the organs of vertebrate animals, is attended by a corresponding increase in the solitary gland-cells of the blood. The office of the *blood-cells*, taken collectively, is that of an immense gland, which elaborates the constituents of the blood. The *fibrin* presents a remarkable index of the vital and intellectual endowments of animals. (p. 59.) It is absent in almost the whole of the invertebrate kingdom; it is soft and unstable in the lowest orders of the vertebrata. The collation of the amount of *fixed saline constituents* in the blood of various animals leads the author to the observation, that their proportion is remarkably uniform throughout the whole animal kingdom.

Regarding the *circulation*, the table giving the proportional weight of the heart

shows that, among the vertebrate classes, this organ is smallest in fishes, and largest in birds. The comparison of the frequency of the heart's contraction in the different classes of animals leads him to the deduction, "that the rapidity of the circulation depends upon the structure, habits, age, and development of animals. If the vital forces are of a low grade, either from original conformation or the depressing influences of old age, the circulation is correspondingly sluggish and feeble." (p. 35.)

Bamberger had the opportunity of observing the contraction and impulse of the heart in a healthy man, thirty years of age, who had inflicted on himself, by means of a knife, a deep wound at the inferior margin of the fifth left rib, somewhat below the nipple and nearer to the middle line. Through this wound, the author believes, the pericardial cavity had been opened, without lesion of the substance of the heart. By introducing the index finger, he could distinctly feel how, with every systole of the heart, the apex, hardened and slightly pointed, moved along the anterior wall of the chest, from above downwards, slightly inclining to the left; and with every diastole, returned to its former position. The duration of the former of these two acts appeared to the author a trifle shorter than that of the latter. He could, however, not perceive either a lever-like movement of the apex towards the front, or a rotation round the longitudinal axis. By this observation, Bamberger was induced to institute, with the assistance of Professor Kölliker, a series of experiments on rabbits, which led to the following results:—1. The change taking place in the shape of the heart with every systole, consists in the shortening of the longitudinal, and in the increase of the antero-posterior diameter; while that from one side to the other becomes probably likewise smaller. 2. The perceptible impulse of the heart is produced merely by the systolic vaulting and hardening of the anterior ventricular wall. 3. An actual locomotion of the heart, in the direction from above downwards, takes place with every systole—as Skoda had observed it in a child without sternum—and at the same time, the large vessels are seen stretched. The elongation of the vessels appears to be the cause of the downward motion of the heart. This inference became particularly probable in a rabbit, in which the sternum had been longitudinally divided, and both halves of the thorax drawn aside; there the pulmonary artery appeared, during the systole, elongated to such a degree, that a piece of it, between two and three lines long, became visible with every systole, and disappeared again with every diastole. 4. The systole is accompanied by a rotatory movement round the axis of the heart, from the left to the right. By the simultaneousness of the rotatory and the descending motion, the heart has the appearance of moving in a spiral direction along the wall of the chest. 5. The heart descends considerably with every deep inspiration, probably in consequence of the stretching of the great vessels. 6. The margin of the left lung, bordering the heart, exhibits two distinct motions: the *respiratory*, showing itself in the gliding down of its inferior margin along the inner wall of the chest with every systole (elongation of the diameter from above downwards), and by the retraction with every diastole; and the *systolic*, which consists in a short and quick movement of the thin anterior border (covering the pericardium), in the same direction and synchronous with the heart, the extent being about one line. 7. The diastolic movements are, as Haller already observed, in every respect the opposite of the systolic. It may be added, that chloroform had been used to facilitate these observations, the inhalation of a few drops having been sufficient to render the respiratory movements and the contractions of the heart so slow, that the single periods could be easily watched.

Chaveau and Faivre have examined the movements of the heart by laying it bare, after having previously divided the medulla between the atlas and occipital bone, resorting at the same time to artificial respiration. They distinguish three periods:—*a.* The systole of the auricles; and simultaneous with this, the diastole of the ventricles. *b.* The systole of the ventricles and diastole of the auricles. *c.* The diastole of both ventricles and auricles. To the second period belongs the first, to the third the second sound. Both sounds are explained by the tension of

the valves. The impulse is attributed to a change in shape, and to an increased firmness of the substance of the heart.

Soire, whose observations are made on frogs, denies the existence of the short pause between the diastole of the ventricles and the following systole. He says that the systole is seen to commence immediately after the diastole, when, in opening the cavity of the chest, loss of blood has been avoided; that the pause in question is observed only when the animals have become anæmic from lesion of large vessels during the operation.

Heidenhain's researches lead him to adhere to the view, that the ganglia are the nervous centres of the heart; that the sympathetic nerve is its real motor nerve; while the medulla oblongata and the pneumogastric nerve have the function of regulating its pulsations. Heidenhain does not confirm Wagner's observation, that the contractions of the heart become more frequent by section of the sympathetic nerve on the neck.

Poiseuille found, by means of injections, that the capillaries of the expanded lung (inspiration) are more stretched, and therefore narrower, than those of the lung containing less air (expiration). He concluded from this, that, during inspiration, the flow of blood in the capillaries of the lungs is rendered slower, and that the whole circulation becomes retarded. This conclusion the author confirms by microscopic observations in the living frog.

III. Skin; Secretion; Excretion; Metamorphosis of Matter.

1. Duriau: *Experiments on the Absorption and Exhalation of the Skin.* (Arch. Génér. 1856, t. ii.; and Prag. Vierteljahrs., vol. iv. p. 53, 1856.)
2. Poulet: *On the Capacity of the Skin to absorb Water and Substances dissolved in it.* (L'Union, No. 33, 1856; and Schmidt's Jahrb., vol. xci. p. 278.)
3. Kletzinsky: *On the Faculty of Diffusion of the Skin.* (Wien Wochenschr., Mai, 1855; and Canstatt, l. c., p. 4.)
4. Schlossberger: *Does the Milk become Acid by Stagnation in the Gland?* (Annal. d. Chemie und Pharm., vol. xcvi. p. 76; and Canstatt, l. c., p. 192.)
5. Gubler: *On the Secretion and Composition of the Milk in New-born Children.* (Gaz. de Paris, No. 15, 1856; and Schmidt's Jahrb., vol. xci. p. 8.)
6. Kölliker and Müller: *Second Report of the Physiological Institution at Würzburg.* (Verhandl. d. Würzb. Gesellsch., vi. 3, pp. 436 ss. 1856.)
7. Schwarzenbach: *On the Presence of Copper in the Human Liver.* (Verhandl. d. Wurzb. Gesellsch, vii. 1, p. 19. 1856.)
8. Jos. Jones: *Digestion, &c.*, l. c.
9. Kaupp: *Contribution to the Physiology of the Urine.* (Vierordt's Archiv, pp 385 ss. 1855.
10. Dunklenberg: *On the Quantity of Phosphoric Acid and Earthy Phosphates in the Urine.* (Liebig's Annal. xciii. p. 88; Canstatt, l. c., p. 202.)
11. Neubauer: *On the Quantity of Ammonia in the Normal Urine.* (Erdmann's Journal, v. lxvi.; and Canstatt, l. c., p. 202.)
12. H. Blot: *On the Physiological Glycosuria of Women in Childbirth, &c.* (L'Union Médic., No. 126, tome x. 1856.)
13. Frerichs and Staedeler: *Further Contributions to the Knowledge of the Metamorphosis of Matter.* (Müller's Arch., p. 37, 1856; and Schmidt, vol. xc. p. 146. 1856.)
14. Frerichs and Staedeler: *On the Metamorphosis of the Cholic Acids into Colouring Matter.* (Müller's Arch., p. 55, 1856; and Schmidt's Jahrb., vol. xc. p. 147.)

The results of Duriau's experiments confirm several of the inferences drawn by Homolle,* who, as well as several other authors, had maintained that the

* See this Journal, No. 27, p. 256. 1854.

skin absorbs water during bathing; Durian proves not only the correctness of this assertion, but shows at the same time, that the amount of the water absorbed changes with the temperature of the bath. He found that exhalation and absorption are almost equal, when the water has the temperature which allows the body to feel it neither cold nor warm. This point is not quite the same in different individuals, but it lies in general between 89° and 94° F. ("point isotherme," "limite thermique.") If the water is warmer, the exhalation exceeds the absorption: the body therefore loses weight. If it is colder, the absorption preponderates: the body gains weight. Both these changes become increased with the increased duration of the bath. In a bath of 71·5°—77° F., the absorption amounted in the average to 16 grammes in a quarter of an hour, to 139 grammes in three-quarters of an hour. At a temperature 105·8° to 107·6°, the loss of weight was equal to 135 grammes after seven minutes, to 378 grammes after fifteen minutes; at 113° F. the loss was as much as 432 grammes after ten minutes.

The inferences regarding the absorption of saline or organic substances dissolved in water, are based on the examination of the urine before and after the use of the bath. Iodide and ferrocyanide of potassium, carbonate of potash, sulphate of quina, and other salts, were employed. The reaction of the urine after the bath was always alkaline, even when nitric acid had been added to the bath. Potash and soda were the only bases found in the urine—no trace of iodine or cyanogen, &c.

Poulet draws the following inferences from his experiments:—1. That the loss of weight of the human body in a bath of 82·4° F. is very trifling after the first hour, but that it never amounts to less than 50 grammes after the second hour. 2. That this loss is produced,—*a*, by increased elimination of water through the lungs; *b*, by the perspiration of parts of the skin not immersed in the water. 3. That the urine becomes alkaline after acid, as well as after alkaline baths.* 4. After friction of the skin with a solution of tartrate of antimony or extract of belladonna, none of these substances were found in the urine. 5. The skin absorbs, therefore, neither water (?) nor substances dissolved in it, as long as the epidermis is entire.

Kletzinsky's experiments likewise confirm the non-absorption of salts through the healthy epidermis. Gaseous or volatile substances, however, may, according to this author, pass through the skin into the body.

Schlossberger, in opposition to Hermbstaedt and Fraas, found the milk always neutral or alkaline, even after long stagnation in the gland—i.e., after between twenty-nine hours and four days in cows, and after between two and eight days in women. Schlossberger, however, has not examined diseased milk; he thinks it probable that in those cases in which the fresh-drawn milk has no acid reaction, it had been secreted in a sour state.

Gubler gives the result of his observations concerning the secretion of a milk-like fluid, which may be squeezed out of the nipples of new-born children, as Morgagni and Natalis Guillot had already mentioned. In 435 children under three weeks of age, the secretion was almost always met with; it was small in quantity, and more serous during the first three days; on the third and fourth day the breasts became turgid, the fluid more abundant, and of an opaque colour; the quantity increased during the following days, and on the tenth day, only one child amongst sixty examined did not yield the fluid; between the tenth and twentieth day there is scarcely any change; the fluid is rarely met with after the lapse of the first month. Regarding the chemical nature of the fluid, the author found it always alkaline—even more so than the milk of nursing women. He gives an analysis by Quevenne, which we subjoin, together with the corresponding figures for the milk of women and asses:—

* See Homolle, Durian.

	Children's milk.	Women's milk.	Asses' milk.
Butter	1·4	2·6	1·4
Casein	2·8	3·9	1·7
Sugar of milk ...	6·4	4·9	6·4
Water	89·4	88·6	90·5
	100·0	100·0	100·0

Saline Ingredients.

Earthy phosphates from the casein	0·120
Soluble salts from the casein	0·040
Soluble and insoluble salts from the sugar of milk	0·180
	0·340

The resemblance of this fluid with the milk of women and asses is confirmed by Donné's lactoscope.

Kölliker and Müller's report contains valuable additions to our knowledge of the *secretion of bile*. The inferences are drawn from experiments performed on dogs. 1. As regards the influence of *meals*, a considerable increase was found from the third hour—the greatest amount, in general, between the sixth and eighth hour, the lowest between the nineteenth and twenty-fifth hour after moderate meals, while the increase continued for sixteen or seventeen hours after large meals. It will be remembered that Arnold found the quantity of bile largest soon after meals, decreasing again after the fourth hour.* 2. The quantity of bile secreted per one kilogramme of dog in twenty-four hours, is estimated at 36·1 grammes, with 1·162 grammes of solid residue: which nearly agrees with the observations of Bidder and Schmidt, while the figures are higher than those given by Nasse† and Arnold. 3. Of physiological as well as pathological interest are the researches made on dogs, after the closure of the external fistula: cases, therefore, in which, as the ductus choledochus had been previously obliterated, *artificial icterus* had been produced. The first signs of the icterus were observed in the urine, and not till several days later it appeared in the conjunctiva and mucous membrane of the mouth. In spite of the most intense icterus, one of the animals remained for several months very lively, and gained weight; sudden death ensued, however, in the midst of apparent health. The examination exhibited the signs of peritonitis, and a perforating ulcer of the duodenum. It follows that the mere retention of bile does not appear to exercise so injurious an influence on digestion and nutrition, and on the nervous system, as is generally assumed. 4. Amongst the *post-mortem phenomena* of dogs affected with biliary fistula, our attention is particularly arrested by the comparative frequency of perforating ulcers of the duodenum, and of incrustation (ossification) of the branches of the cœliac axis and the mesenteric artery. Two of the five dogs experimented upon died of these perforating ulcers; a third of them manifested the signs of gastro-intestinal catarrh. The incrustation of the arteries was discovered likewise in two out of the five dogs, and has not been looked for in the remaining three. These pathological alterations, which had been formed without obvious symptoms during life, must make us careful in asserting that the bile may be drawn off without material injury to the constitution; the more so, as all the dogs experimented upon, as well by the authors as also by Schwann,‡ Nasse, and Arnold, died more or less suddenly, although they had been well provided with food. Regarding the etiology of the incrustation of the arteries, we must wait for further observations; we have already other pathological facts before us which make it probable that diseased states of the bile-conducting apparatus are apt to cause this morbid condition of the bloodvessels of the intestines.

* See this Journal, No. 33, p. 232.

† Commentatio de Bilis quotidie a Cane secreta copia et indole. Marburgi, 1851.

‡ Müller's Archiv. 1844.

Schwarzenbach confirms the existence of *copper* in the human liver, but he found only 0·009 grammes of oxide of copper—i. e., 0·004 of pure copper in 2100 grammes of liver; while Orfila had found ten times as much. The same quantity of the same liver yielded about 0·017 grammes of pure lead.

Regarding the physiology of the *pancreas*, Jones defends the assertion of Bernard, that its chief office is to prepare fatty matters for absorption, adducing in favour of this view the following facts:—1. In the garfish (Lepidosteus osseus), the emulsion of the fatty matters takes place in the duct and cæca of the pancreas and their immediate vicinity, and nowhere else in the alimentary canal. 2. The pancreas of carnivorous, is relatively much larger than that of frugivorous and granivorous animals; the amount of oil consumed by the former is much larger than that consumed by the latter. The size of the pancreas amongst carnivorous animals is in a measure proportional to the amount of the oleaginous matter consumed. 3. The pancreas of carnivorous chelonians, fed upon vegetable matters, degenerated in its structure.

Kaupp's observations, performed with much attention to diet and manner of living, show that, as a general rule, the quantity of *chloride of sodium* excreted by the *urine* is in proportion to the quantity of this salt ingested with the solid and fluid nutriments. If, however, after several days of abstinence from culinary salt, it is introduced in larger quantities, the amount excreted through the kidneys is smaller than that ingested; but this is only an apparent exception of the rule, as the body seems to store up a certain quantity, of which it had been deprived by the preceding abstinence. Concerning the connexion between the ingestion of culinary salt and the excretion of other constituents of the urine, Kaupp's tables show, that the increased ingestion of salt causes an increased excretion of water, and also of urea, while the other solid substances exhibit a decrease.

Dunklenberg found the quantity of *phosphoric acid* contained in the urine of twenty-four hours varying between 2·144 and 2·657 grammes; that of the *earthy phosphates* between 0·763 and 0·972 grammes.

According to Neubauer's examinations, the urine of a healthy young male person contained in twenty-four hours, in the average, 0·8351 grammes of *ammonia*, that of another likewise healthy man 0·6137 grammes, which figures correspond to 2·6361 and 1·9305 grammes of hydrochlorate of ammonia. The highest figures in the two subjects were 3·8038 and 2·3025 grammes; the lowest, 1·4272 and 1·5987. Exercise appeared to have no marked influence, while increased ingestion of water was accompanied by increased excretion of ammonia.

H. Blot, of the Clinique d'Accouchements de la Faculté de Paris, has made the interesting observation, that the urine of all lying-in, of all nursing, and of about half of the pregnant women, contains sugar. The glucose commences, in general, to appear as soon as the secretion of milk is established, sometimes even before this. The quantity secreted in twenty-four hours is not given, but it is stated to be much smaller than in diabetes. The urine of the best nurses was found richest in sugar. The glucose begins to disappear from the urine as soon as lactation ceases either permanently or becomes transitorily suspended from disease or other causes. Blot infers, therefore, that this glycosuria is in an intimate relation with the secretion of milk. He further adds the observation, that the same physiological phenomenon exists also in the cow.

Frerichs' and Staedeler's further researches on the occurrence of *leucin* and *tyrosin* in the animal body, lead to the following inferences:—1. That leucin and tyrosin in some diseases and functional disorders of the liver, accumulate in this organ in considerable quantity, while they are absent in the normal state. 2. Leucin is always found in the juice of the spleen; tyrosin not as regularly. 3. Both are met with in the pancreas; no other organ contains leucin in so large a proportion. 4. The salivary glands do not yield so much leucin as the pancreas. 5. The lymphatic glands exhibit an ample amount of leucin, but no tyrosin. 6. The examination of the thyroid gland and of the thymus of calves, ten weeks old, leads to the same result. 7. In the brain, leucin does not appear constantly

to occur. 8. In the muscles and lungs it was not discovered. 9. The urine of typhus patients exhibited leucin in large amount, and also some tyrosin.

The authors are of opinion that these two bodies are the products of albuminous substances; that they are formed principally in those glands which contain a ferment-like element; that they are thence carried by the circulation to other organs, especially the liver, where they are farther decomposed, entering into the formation of bile, perhaps also volatile fatty acids.

The experiments made by the same authors on *glyco-cholalic acid* and *glyco-cholalate of soda*, when acted upon by sulphuric acid, lead them to the conclusion, that the acids of the bile may be transformed into colouring matter of different shades, according to the degree of temperature employed and other circumstances. The following experiment makes it probable that the same metamorphosis may take place in the living organism. A drachm of colourless ox-gall dissolved in distilled water was injected into the vein of a dog; three ounces of urine were obtained six hours later; this urine formed a considerable green sediment, which exhibited under the microscope green granules, and with nitric acid the characteristic change of colour.

IV. Animal Heat.

Cl. Bernard: *Experimental Researches on Animal Heat.* (L'Union Médic., tome x. No. 108. 1856.)

In order to ascertain the influence exercised on the temperature of the blood during its passage through the digestive apparatus, Bernard examined:—*a*, the temperature of the abdominal aorta—i.e., of the blood before its distribution to the digestive apparatus; *b*, of the portal vein—i.e., of the blood after its passage through the intestinal canal, spleen, pancreas, &c., but before its entrance into the liver; *c*, of the hepatic veins—i.e., after its passage through the liver and the whole digestive apparatus. From these necessarily very delicate experiments, for the description of which we refer to the original, the author draws the following conclusions:—1. The heat of the blood is constantly increased by its passage through the digestive apparatus, in such a manner that it is warmer in the portal vein than in the abdominal aorta, and still more so in the hepatic; the process of digestion exercising apparently no influence over this phenomenon. 2. The blood of the hepatic veins is a constant source of calorification for the blood conveyed through the inferior cava to the heart. This may be considered even as the principal source, for nowhere else is the blood found so warm as in the hepatic veins, where it raises the thermometer in vigorous dogs even to 106°·88 Fahr. 3. Among the organs which contribute to the augmentation of the temperature of the blood in its passage through the digestive apparatus, the liver maintains the highest rank. This organ must therefore be regarded as one of the principal sources of animal heat.

V. Nervous System.

1. Arnsperger: *On the Cause and Pathological Nature of the Alteration of the Lungs after the Section of the Nervi Vagi on the Neck.* (Virchow's Archiv, vol. ix. 1, 2, 3. 1856.)
2. Fano: *Section of the Right Nervus Vagus in Man.* (Arch. Génér., Févr. 1856; and Schmidt's Jahrb., vol. xci. p. 19. 1856.)
3. Budge: *On the Movements of the Iris.* (Braunschweig, 1855; and Canstatt, l. c., p. 116.)
4. Schiff: *Researches on the Physiology of the Nervous System.* (Frankfurt, 1855; and Canstatt, (l. c., p. 133.)
5. Heidenhain: (l. c., cf. ii.)

Arnsperger distinguishes the effects of the section of the recurrent branch alone from those of section of the entire pneumogastric nerve. After division of the

recurrent laryngeal nerve, he observed:—1. Narrowing and even closing of the rima glottidis. 2. Relaxation of the ligaments of the glottis. 3. Change in the number and depth of respirations. 4. Incomplete deglutition, allowing the passage of particles of food into the trachea of rabbits. 5. Death through inflammatory affection of the lungs in rabbits. 6. Loss of voice as the only consequence of this operation in dogs, the larynx of which does not permit the entrance of particles of food into the bronchi. Regarding the section of the *pneumogastric nerves* on the neck, the author remarks:—1. Division of one nerve causes no functional derangement of importance. 2. Division of both nerves causes as well functional disorders as also a series of pathological alterations. 3. The former consist in loss of voice, retardation in the respiratory movements, acceleration of the contractions of the heart. 4. The anatomical alterations are:—Consolidation of the parenchyma of the lungs, serous exudation, emphysema, sometimes coagulation of the blood in the bloodvessels of the lungs. 5. Death, with very rare exceptions, in rabbits sooner than in dogs. Only one dog survived the operation by rapid reunion of the dissected nerves.

The principal anatomical alteration after section of the *recurrent* nerves consisted in lobular inflammation (catarrhal or broncho-pneumonia, or "pneumonie lobulaire mamelonnée" of Rilliet and Barthez), tending to the formation of abscess round the foreign bodies.* The condition of the lungs after section of the *pneumogastric* nerves is described as bronchitic condensation ("bronchitische Verdichtung" of Hasse), a state analogous to, but not quite identical with, atelectasis. In consequence of the diminshed respiratory action, the vesicles collapse; this collapse leads to stasis and serous exudation (œdema). The analogy of this state with atelectasis, and its difference from inflammation, is shown by the redness, the condensation, the firmness, and the sunken state of the affected tissue, its irregular distribution over the lobes, and, lastly, its capability of being inflated.

Fano describes the details of the case of a man, aged forty-five, in whom, together with a carcinomatous tumour on the right side of the larynx, a piece of the pneumogastric nerve was removed. The only symptoms attributable to this lesion, were increased hoarseness and difficulty of expectoration, which is quite in accordance with the observations made by Longet, Horner, and Robert.

Budge's valuable contribution to the physiology of the *iris*, contains the results of the author's own researches, as well as those of other physiologists. We can here of course give only a few of the principal results. Budge attributes the sensation of the iris exclusively to elements from the *fifth* pair, questioning the influence of the optic, oculo-motor, and sympathetic nerves. Irritation of the *third* pair, in mammalia and birds, produces contraction, that of the *sympathetic*, dilatation of the pupil; galvanism applied to the iris itself causes in birds rapid general contraction, while this is slower in birds. The reflex action which leads to contraction of the pupil, in consequence of irritation of the optic nerve, takes place by the medium of the *corpora quadrigemina*, after the extirpation of which, this reflex action is no longer observed. Section of the *fifth* nerve, as well before as after the ganglion Gasseri, causes contraction of the pupil of the same side. Budge concludes from this, that the first branch of the nervus trigeminus contains motor fibres for the sphincter of the iris. The author describes two central organs of the iris in the *medulla*: (*a*) the centrum cilio-spinale inferius, situated, in rabbits, between the sixth cervical and the fourth dorsal vertebra; extirpation of this part causes contraction, irritation causes dilatation of the pupil; irritation of either of the lateral halves, when separated by a longitudinal section, causes dilatation of the corresponding pupil. The fibres to which this influence is attributed, leave the medulla by the anterior fasciculi. (*b*) The superior centre of what Budge calls the "iris sympathicus" lies near the point of exit of the ninth pair, and is connected with the superior cervical ganglion by the anastomotic fibres between the ninth pair and the sympathetic nerve. Budge denies that the vagus exerts any influence on the iris.

* See this Journal, No. 33, p. 235. 1856.

Concerning the influence of the *distance of shining bodies* on the *width* of the pupil, Budge's experiments show, that the degree of contraction of the iris is not in a direct proportion to the distance of the shining body; the distances being in the proportion of 1 : 2 : 3, the diameters of the iris were 1 : 1·096 : 1·165. Similar experiments on the influence of a varying intensity of light on the width of the pupil, gave for the intensities of 30 : 9 : 4 : 2, the diameters of the pupil, 3 : 3·47 : 3·94 : 4·73.

Schiff presents the result of his experiments on the function of the fifth pair, of the sympathetic nerve, and on the influence of paralysis of nerves on the increase of animal heat. As we have no space for a condensed extract of all three subjects, we offer only a few points from the third of them. The increased fulness of the bloodvessels after the section of nerves, is attributed to the paralysis of the vaso-motor nerves; the elevated temperature is considered to be caused by the presence of the larger quantity of blood. We have related, in former Reports, the experiments of other observers regarding the change of the temperature of the head following the section of the sympathetic nerve on the neck.* Schiff shows that the sympathetic is not the only vascular nerve of the head, that the nervus "auricularis cervicalis" always supplies a part of the vessels of the ear; section of this nerve always causes an increase of warmth in the corresponding ear, which, however, disappears after some days; galvanic irritation of the sympathetic nerve, and of the auricularis respectively, cause contraction of a different set of vessels. The fifth pair sends fibres to the vessels of the conjunctiva, the mucous membrane of the nose, and the gums; its section causes slight increase of temperature in these parts. Section of the facial nerve, several days after the extirpation of the superior cervical ganglion, gives rise to a further augmentation of warmth in the ear in rabbits; it must therefore contain vaso-motor fibres, which are not derived from the sympathetic, but from the anastomosis with the pneumogastric nerve, as section of the facialis immediately in front of the stylo-mastoid foramen is not followed by any rise of temperature. Section of the ischiatic nerve occasions the corresponding foot to become warmer than that of the other side; a similar effect is produced on the hand and forearm by section of the brachial plexus. Schiff also gives a series of experiments showing the influence of the nervous centre on the tone of vessels. Thus section of either half of the cervical portion of the medulla, on any spot whatever, effects increased heat of the ear of the corresponding side; at the same time, the arterial pulse is felt larger and fuller. The author confirms Budge's view, that the vaso-motor nerves of the head contained in the sympathetic nerve, are derived from the medulla; those of the thighs appear to have their origin in the dorsal medulla.

VI. Senses.

1. Krause: *On the Refractive Indices of the Transparent Media of the Human Eye.* (Hannover, 1855; and Canstatt, l. c. p. 29.)
2. Helmholtz: *On the Accommodation of the Eye.* (Graefe's Archiv, vol. i. 1855; and Canstatt, l. c. p. 12.)
3. Dubrunfaut: *Observation on Vision.* (Comptes Rend., vol. xli. p. 1087; and Canstatt, l. c. p. 12.)
4. Budge: (l. c. cf. Nervous System).
5. Kramer: *Contribution to the Physiology of the Human Ear.* (Deutsche Klinik, Sept. 1855; and Canstatt, l. c p. 115.)
6. Czermak: *Physiological Studies. Section II.—Sense of Touch.* (Sitzungsber. d. Wiener Academ., März, 1855; and Canstatt, l. c. p. 124.)

Krause measured the refractive indices of the eyes of dead human bodies, after having previously convinced himself on the eyes of animals, that there is scarcely

* See No. 33, p. 231; and No. 35, p. 230.

any difference between the figures found immediately after death, and those found at the period when the human eyes were examined by him. Thus he obtained, as the average of twenty experiments, the refractive index of the cornea = 1·3525 (that of distilled water being 1·3358); the index of the exterior lamellæ of the crystalline lens, 1·4071; that of its middle lamellæ, 1·4319; of the nucleus lentis, 1·4564; of the humor vitreus, 1·3506; of the humor aqueus, 1·3435.

Helmholtz examined, by an ingenious apparatus, for which we must refer to the original, various phenomena connected with the process of accommodation. No alteration is to be observed in the curvature of the cornea during the accommodation for different distances; the iris becomes more prominent by the accommodation to near objects; the radius of the curvation of the anterior surface of the crystalline lens is, during accommodation for near objects = 8·6 millimètres; for remote objects = 11·9 millimètres. During the accommodation for near objects, also, the radius of the curvation of the posterior surface appears to become diminished; while, therefore, the lens becomes more curved on both sides, the posterior vertex remains in its place, while the anterior advances considerably. The lenses of dead bodies have the shape of those adjusted for near objects; they are in general even thicker than those. Helmholtz is therefore of opinion that this is the form of the lens in its state of equilibrium. Regarding the influence exercised by the iris in this alteration of shape and position of the lens, Helmholtz maintains a similar view to that of Cramer.*

Dubrunfaut calls attention to the circumstance, that bright objects are not seen brighter with both eyes than with one. He explains this by the observation, which he believes to have made, that if we close one eye, after having looked on a bright object with both eyes, the pupil of the other becomes dilated exactly to such a degree that its entire area becomes doubled. Thus the central organ would receive through one eye the same amount of impression as it had previously received through both eyes.

The *cartilaginous part* of the *external ear*, according to Kramer, leads into the external meatus, the third part of all the acoustic waves reaching the tympanum. If we cover the external ear, with the exception of the entrance in the meatus, by means of wet flannel, the sounds of a watch perceived by the uncovered ear at a distance of twenty-one to twenty-two inches, are heard only when not further removed than between thirteen and sixteen inches.

Czermak's experiments, made according to Weber's method, by means of a pair of compasses, show that the *sense of space* (i.e., of touch) is in boys more delicate than in grown-up persons, the differences being greatest in those parts of the skin where the sensibility is least acute. There is, however, also amongst children a great difference. Czermak's experiments on blind persons confirm the common belief, that the sense of touch, on the whole surface of the body, is considerably more developed in the blind than in those who see.

VII. The Supra-renal Capsules.

Brown-Séquard: *Experimental Researches on the Physiology and Pathology of the Supra-renal Capsules.* (L'Union Médicale, tome x. No. 108. 1856.)

Brown-Séquard, induced by Addison's work on the supra-renal capsules, made experiments relating to the physiology and pathology of the supra-renal capsules, on rabbits, dogs, cats, and guinea-pigs. These organs, the author infers, are essential to life, at least in the animals just named, as none of those experimented upon survived the extirpation of either one or both of the capsules for many hours. The symptoms—which were the same in almost all the animals—consisted in a considerable sinking of strength, in various derangements of the respiration and circulation, in convulsions, turning round (*turnoiement*), delirium, and coma. The

* See this Journal, No. 29, p. 272. 1855.

convulsions are, like those from strychnia, easily excited by reflex action. The extirpation of only one organ is frequently followed by more violent convulsions of the other side, and often also by a more contracted pupil of the side operated upon. Death follows more rapidly upon this operation than upon extirpation of the kidneys. The author attributes to the supra-renal capsules a near relation to the cerebro-spinal nervous centre. He ascribes the symptoms enumerated in part to the lesion of several filaments of the great sympathetic nerve going to these bodies, but this alone he considers insufficient to account for the rapid death after the extirpation.

Brown-Séquard adds, that he has frequently met with acute inflammation of the supra-renal capsules in rabbits, always accompanied with a rapidly fatal termination, preceded by symptoms similar to those consequent on extirpation. He has further witnessed the occurrence of congestion and hypertrophy, in some instances also of inflammation of these organs, after the dissection of the lower part of the dorsal and the upper part of the lumbar medulla.

VIII. Generation and Development.

1. Dareste: *On the Influence of Impermeable Coating of the Shell of Eggs on the Development of the Chicken.* (Comptes Rend., Nov. 1855; and Canstatt, l. c. p. 154.)
2. Küchenmeister: *On the Cœnurus Cerebralis of Sheep.* (Bulletin de l'Acad. de Bruxell., 1855; and Canstatt, l. c. p. 148.)
3. Van Beneden: *On the Development of Cœnurus Cerebralis in Sheep.* (Bullet. de l'Acad. de Bruxell., 1855; and Canstatt, l. c. p. 148.)
4. Ankermann: *De Motu et Evolutione Filorum Spermaticorum Ranarum.* (Regimonti, 1854; and Canstatt, l. c. p. 149.)
5. T. Moleschott and Richetti: *On means to Revive the Spermatozoa of Animals.* (Wien Med. Wochenschrift, Mai, 1855.)
6. Kölliker: *On the Vitality and Development of Spermatozoa.* (Würzburg Verhandlungen, vol. vi. 1855.)
7. Kölliker: *Physiological Studies on the Spermatic Fluid.* (Zeitschr. für Wissensch. Zool., vol. vii. 1855.)

Dareste in his experiments applied varnish to different parts of eggs, and at different periods of incubation. When the thick end of the egg had been varnished at the commencement of the incubation, the chicken-embryos were either destroyed, or when they had remained alive, the allantois was found attached to an unvarnished part of the egg. The author thus corroborates the view, that the allantois forms an organ of respiration.

Küchenmeister confirms the observation, that the tænia cœnurus of the intestines of dogs, is the full-grown individual of the cœnurus cerebralis of sheep. The sturdy of the sheep is propagated by the circumstance, that the heads of sheep thus affected are taken as food by dogs, in the intestines of which tænias are developed; the excrements of these dogs, containing the proglottides filled with eggs, are deposited in the grass on which the sheep feed. Damp meadows are particularly favourable to the development of the cœnurus, because they prevent the proglottides and eggs from becoming dry.

Van Beneden, who produced the sturdy in sheep by feeding them with proglottides, found in the heads of these sheep not only the passages of the cœnurus, but also the animals themselves in the cortical substance of the brain in different stages of their development.

Kölliker, Ankermann, Moleschott, and Richetti have made very careful researches on many substances influencing the motion of the spermatozoa of various animals. Serum of blood, lymph, the secretion of the prostatic gland, of Cowper's glands, a solution of albumen, &c., favour the motion of the spermatozoa.

Bile, milk, mucus, if not too viscid, do not prevent the motion; while this is checked by viscid mucus from the collum uteri. The just-named fluids, however, when too much diluted, act like water—i.e., they then cause the motion of the spermatozoa to cease, without destroying their vitality. Amongst the solutions of alkaline and earthy salts, there are some that act favourably when little concentrated, as the chlorides of sodium and of potassium, and nitrate of soda (1%); others, which act injuriously when so much diluted, while they become innocuous or even favourable to the motion by greater concentration (5%), as phosphate of soda, sulphate of soda, and sulphate of magnesia. The caustic alkalies in much-diluted solution possess the greatest power in effecting motion, even where it had long ceased. Concerning the manner in which these substances act, Ankermann is of opinion that the motion is due to the process of diffusion; Kölliker seems disinclined to ascribe this phenomenon merely to endosmosis or imbibition, but attributes to it a vital character. Mineral acids, metallic salts, and narcotics, act in a decidedly injurious manner.

HALF-YEARLY REPORT ON MATERIA MEDICA & THERAPEUTICS.

By Robert Hunter Semple, M.D.,

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I. *On the Therapeutical Applications of Glycerine.* By Dr. W. Lauder Lindsay. (Edinburgh Medical Journal, September, 1856.)

Dr. Lindsay used glycerine himself, to the extent of two or three teaspoonfuls daily for several weeks, in order to test its nutrient properties. He found the most palatable mode of using it was when it was mixed with coffee. The result was, a gain of weight to the extent of two pounds at the end of four weeks; and on discontinuing the glycerine, the weight gradually fell. The glycerine is readily miscible with fluids of all kinds. Coffee may be sweetened by it instead of sugar; and if the somewhat peculiar taste which it imparts should be objectionable to fastidious stomachs, a small quantity of sugar may be superadded. It has the characters of a syrup, and does not betray its presence by oil globules or otherwise. It may be added to tea, and it sweetens milk and cream very pleasantly; but its mixture with water is very palatable, and is the readiest and cheapest mode of administration. Dr. Lindsay carefully observed its effects as a nutrient and alterative in eight patients, to whom it was given in doses of two or three tea or table-spoonfuls daily for a month. All the patients before taking it were more or less anæmic, emaciated, and feeble; in all the diet, exercise, and occupations were otherwise the same. At the end of the month all of them appeared greatly improved in their general condition; they seemed plumper and stronger, and in some the countenance was even ruddy. In most of the cases there was a marked increase in weight at the end of the month.

Dr. Lindsay has also given glycerine internally in a variety of affections, in combination with several alteratives and tonics, such as iodine, iodide of potassium, quinine, and iron, or as the basis of expectorant or demulcent mixtures. It was found to answer extremely well as a solvent or suspending agent, or a vehicle. The author thinks that all the alteratives or tonics which have recently been combined with cod-liver oil, might be administered more agreeably if dissolved or suspended in glycerine. Such are iodine and quinine; the iodide, lactate, and bromide of iron; the protiodide, biniodide, and bichloride of mercury; the iodides of arsenic and sulphur; and the valerianate of zinc. By the majority of patients, to whom it was given as a nutrient, it was much relished; and its sweet taste would probably render it a favourite with children. The advantages of glycerine over cod-liver oil consist in its pleasant sweetness, and its freedom from all disagreeable odour; in its ready solubility in, or miscibility with, ordinary fluids; in the absence

of the principles which, in animal and vegetable oils, so frequently nauseate and purge; and in its solvent and other properties, which render it useful as a vehicle or basis for pharmaceutical preparations. Its great disadvantage is its present comparatively high price. In opposition to Dr. Garrod, who has suggested that cod-liver oil acts simply in virtue of its oleine, Dr. Lindsay believes that glycerine is the active principle. Glycerine appears already to have been tried somewhat extensively in phthisis as a substitute for cod-liver oil, but the results described hitherto are contradictory. In other strumous cases, however, it appears to have proved serviceable.

Dr. Lindsay has used glycerine frequently as a dressing to wounds, ulcers, and abrasions of various kinds, with marked good results; and in these respects it seems to be equal, or even superior, to collodion. In the treatment of bed-sores, he regards it as superior to gutta percha, but inferior to collodion. In the treatment of skin diseases it has been found useful, not only by keeping the skin constantly moist, but by allaying the irritation which so frequently accompanies cutaneous eruptions.

Glycerine may also become very serviceable in pharmacy; and particularly in the preparation of extracts, pills, syrups, and infusions, it promises to be very useful. It has been proposed as a substitute for syrup in such cases as the syrupus ferri iodidi; and as a vehicle for medicines, it combines the properties of a syrup and a mucilage.

II. *On the Endermic Application of Iodide of Glycerine.** By Dr. FERDINAND SZUKITS. (Wochenblatt der Gesells. der Aerzte zu Wien. Sept. 1, 1856.)

The author of this paper, after enumerating the several forms in which iodine has hitherto been endermically applied, proceeds to remark, that all the solvents in ordinary use take up only a small quantity, with the exception of alcohol. It was therefore desirable to discover a solvent which, without affecting the skin like the alcoholic tincture, should take up as large a quantity as possible of the iodine. This solvent was found, in 1854, by Cap, in glycerine. Cap attributed to glycerine the part of a simple solvent, and he proposed it, among others, for the solution of bromine, iodine, oxyde of lead, strychnia, veratria, atropia, morphia, &c. To Dr. Richter belongs the credit of having first introduced into practice the solution of iodine in glycerine. He combined the iodine with iodide of potassium in order to facilitate the solution of the former; combined with this, it may be dissolved in any quantity up to the proportion of almost three to five. But in this concentrated state it is a caustic solution, and too strong for common endermic use; and the author has proposed a proportion of one part of iodine and five parts of glycerine, as a solution which may be applied for a long time to the parts about the neck and to the female breast, without any inconvenience except a slight burning. In the neck and the female breast, the application, after two or three paintings, causes smart burning; and after four or five it produces more or less large excoriations, which require the discontinuance of the remedy and the application of cold fomentations. On the abdomen and in other parts, these symptoms occur much later. After a longer application of the iodide of glycerine, the epidermis peels off on the painted parts. The paintings were performed once a day in the author's cases, and paper of gutta percha was laid over the painted places to prevent evaporation. The paintings may be continued for a month without producing *iodism*, and without causing the slightest disturbance in the well-being of the patient. According to the experiments of Bonnet, the absorption and elimination of iodine may take place to the amount of a gramme of iodine (15·4 grains) per diem for several weeks, without any injury to the general health. The number of the cases in which Dr. Szukits has employed the iodide of glycerine were 24, in some of which the most satisfactory results were obtained.

* See British and Foreign Medico-Chirurgical Review, July, 1856, p. 239.

III. *On the Effects of the Tincture of Iodine applied locally on the Mucous and Serous Membranes, in relation to Pain.* By Dr. BOINET. (L'Union Médicale, June 14th, 1856.)

Dr. Boinet remarks that the contact of tincture of iodine with the mucous membranes is not at all painful; and that it is possible to paint, almost without the consciousness of the patients, the pharyngeal and buccal mucous membranes, the tonsils, the neck of the uterus, the vagina, &c., without causing any pain: on condition, however, of not allowing the tincture to touch the orifices of the mucous cavities—namely, the points where the mucous membrane terminates and the skin commences; for the pain is very severe, and is prolonged for a considerable time, whether the tincture is applied to the lips, the anal orifice, or the female external parts of generation. In these cases the patients experience a pain as intense as when the tincture of iodine is applied to the skin denuded of its epithelium, or to a recent wound. There is the same pain when the ocular or palpebral conjunctiva is touched for the treatment of certain inflammations of the eye, the removal of granulations, &c. If several successive paintings take place, the same change ensues on the mucous membranes as on the skin—namely, that desquamation having taken place, the pain becomes then very severe after the subsequent application. As to the serous membranes, the tincture of iodine always produces in them very severe and cutting pains, and in an instantaneous manner. But this pain is much less severe upon the articular membranes than on the peritoneum. The acute pain produced by the contact of the tincture of iodine with the peritoneum is, in fact, a certain sign which indicates that an ascites has been mistaken for an ovarian dropsy; inasmuch as, in the latter affection, the iodine injection is never painful. This pain is also a proof, when it arises with less intensity in injecting an ovarian cyst, that a certain quantity has penetrated into the peritoneum.

IV. *On the Febrifuge Properties of Apiol.** By Dr. JORET. (L'Union Médicale, June 26th, 1856.)

The author of this paper considers that apiol possesses all the advantages of arseniate of soda in the treatment of intermittents, without the inconveniences which often attend the use of the arsenical compounds; that the safety of its action is also quite manifest, and that the dose of it may be increased without fear of producing any other effects than those which are habitually observed after the administration of the sulphate of quinine. Three cases are adduced in support of the statements advanced, and care was taken not to administer the febrifuge until the fever was completely developed, nor until the succession of the fits, each time brought nearer together, caused a fear of greater severity in the fits about to follow. The author remarks that the sulphate of quinine will long remain the best anti-periodic, and that recourse must always be had to it by preference whenever the object is to cut short a dangerous intermittent, the approaching paroxysm of which might be fatal; but he remarks that the apiol acts with the same safety, that it may be administered with the same advantage, in all fevers where it is not of much importance to put a stop to the paroxysms a day earlier or a day later, and that there is nothing to offer any obstacle to its therapeutical employment.

V. *On the Rottlera Tinctoria as an Article of the Materia Medica.* By Assistant-Surgeon THOMAS ANDERSON, M.D. (Indian Annals of Medical Science, No. 5, October, 1855.)

The *rottlera tinctoria* is a species of euphorbiaceous plant found in the hilly parts of India, as along the base of the Himalayas from Assam to near Peshawur,

* See British and Foreign Medico-Chirurgical Review, Jan. 1856, p. 247.

in Central India, at the Northern Cercars, in Mysore, and at Parell Hill, near Bombay. In its habit it is almost arborescent, growing to twenty or thirty feet high. The substance called kamila, obtained by brushing the powder off the capsules of this plant, has long been known in India as a dye, and it is also occasionally used by the natives as a vermifuge; this latter property is supposed by Dr. Royle to depend upon the stellate hairs found in the powder. Dr. Anderson mentions that his attention was first called to the medicinal properties of this substance by Dr. Gordon, of the 10th Regiment, who had met with great success in employing it as a remedy for tapeworm. Dr. Anderson afterwards employed it himself for the expulsion of the same parasite in the case of several men of his own regiment. The powder is of a dark brick-red colour, with a peculiar heavy odour, increased on its being rubbed between the fingers. Its physiological action is very simple: on an adult the powder in a dose of ʒij. or ℥ss., besides purging, very often causes nausea and vomiting, and in some cases griping; its action on the bowels, however, is very variable, producing from four to ten or fifteen stools even when a dose of ʒiij. has been administered. A strong ethereal or alcoholic tincture, besides acting more mildly, is followed by more uniform effects. Dr. Anderson found that an amount of the tincture sufficient to produce the full anthelmintic effect of the drug was never followed by more than six stools, and always acted without griping. After ʒiij. of the powder have been administered, the worm is usually expelled in the third or fourth stool. It is generally passed entire, and almost always dead, and in about fifteen cases examined by Dr. Anderson he was unable to detect the head. The vermifuge properties of *rottlera tinctoria* have been attested in a large number of cases. Dr. M'Kinnon has mentioned sixteen successful cases in a paper published by him, and he has since administered the powder to nearly fifty patients, out of whom there were only two cases in which no worm was expelled. Dr. Gordon has tried the remedy in thirty cases of tapeworm with uniform success. The dose of the powder of the kamila which seems to act most satisfactorily is ʒiiss. to ʒiij. in an adult; and ℥ss. of the alcoholic tincture is the dose which is followed by the most successful effects.

VI. *Poisoning by Strychnia successfully Treated by Camphor.* (American Journal of the Medical Sciences, October, 1856.)

Professor Rochester has communicated to the Buffalo Medical Association the case of a person, aged thirty-two, who had taken strychnia for the purpose of self-destruction. He stated that the quantity taken was four grains. When brought into the hospital he had several tetanoid convulsions. A large sinapism was directed to be applied to the epigastrium, and two grains of powdered camphor were given with half a teaspoonful of tincture of camphor suspended in water. The sinapism had hardly been applied and the camphor taken, when a spasm commenced, first showing itself in the cervical muscles, then in those of the arm and chest, the latter producing slight opisthotonos, and lastly in those of the face, turning the eyes into their orbits, and setting the lower jaw firmly. The pulse was eighty-eight, and regular; respiration seemed to be entirely suspended; no respiratory murmur was detected, but the heart's sounds were quite audible. The paroxysm lasted about three minutes, and at its termination the camphor was repeated, with the addition of half a grain of morphia. About half an hour after the paroxysm just described, the patient was seized with another, and the camphor was directed to be given every fifteen minutes. The spasms returned at intervals, but they finally ceased about three hours after his admission into the hospital. The next day he was much better, had had some sleep, and said he was hungry. The camphor, of which he had taken about ʒj., produced neither cerebral nor gastric derangement. Dr. Rochester remarked that this was the second case reported by him this year where camphor had been successfully employed to counteract the effects of strychnia, and he thought that there was no doubt as to its properties as an antidote.

VII. *On the Therapeutical Action of the Galvanic Current on the Human Nerves and Muscles.* (L'Union Médicale, October 4th, 1856.)

M. Remak of Berlin, in a series of experimental researches, has found that the application of a galvanic current to contracted muscles rendered them softer and more obedient to the will. He therefore directed his attention to the therapeutical effects produced by the galvanic current upon rheumatic contractions, as well as those which are combined with cerebral hemiplegia. In the course of his researches, he observed several times that the paralysis of the face or of the tongue, or even intellectual weakness, was benefited by the currents, although they had been conducted only through the extremities; and he was therefore convinced that the action of the current was transmitted to the nervous centres. He accordingly tried its operation in the cure of partial and general chorea, and then of certain cases of paraplegia, and of paralysis of the bladder and the rectum. The greatest success has attended this treatment, but as the number of patients does not yet exceed two hundred, M. Remak is not at present prepared to enter into the details of the methodical application of constant currents in several diseases; but he throws out a suggestion that the galvanic current may eventually be found beneficial in the cure or amelioration of spinal distortions, and of the shrinking of the pectoral cavity, which so often arises in youth by contraction and weakness of the respiratory muscles.

VIII. *On Hyoscyamia.* By Professor Schroff. (Wochenblatt der Zeitschrift der Gesellschaft der Aerzte zu Wien, June 16th, 1856.)

The hyoscyamia employed by Professor Schroff was supplied to him by Meek of Darmstadt, and presented the following properties: it was not crystallized, but presented an amorphous form of crystal, inasmuch as it formed a half-transparent, tough, viscid mass. It presented a yellowish-brown colour, and smelt strongly of the æther and alcohol which were used in its preparation, and had a sharp, biting, nauseously bitter taste. It was gradually dissolved in water by trituration, and also in alcohol and ether, but unchanged by exposure to the air. Concentrated nitric acid dissolved it without changing its colour. When sulphuric acid was poured on it it swelled up, became coloured reddish-brown at the edges, and dissolved into a brown solution. The solutions in acids and the salts were precipitated by tannic acid. The result of the experiments made by Professor Schroff on rabbits proves that hyoscyamia is a poison to these animals, although nearly twenty times the quantity of a very powerful alcoholic extract of the seeds of henbane does not injure them. This circumstance therefore proves the great affinity which exists between hyoscyamia, atropia, and daturia; atropia given to two rabbits caused death, after inflammation of the lungs, while a considerable quantity of the most powerful powdered belladonna did not hurt them. The appearances presented during life by animals treated with hyoscyamia and with atropia, were the same as concerns the inflammation of the lungs, but there were some differences in their respective operation upon the nervous system. In poisoning by hyoscyamia the animals remained quiet, and exhibited no effort to move, which was not the case with animals poisoned by atropia. In the physiological experiments on the human subject with atropia and daturia, the efforts to move were uncommonly heightened, and even amounted to a desire for fighting, in spite of the weakness of the extremities; but neither large doses of the preparations of henbane, nor of hyoscyamia, produced any inclination to increased movement; but, on the contrary, there was a great propensity to sleep, which was subsequently fast and deep. In henbane, also, there was wanting the inclination to delirium, such as resulted from the operation of the other two alkaloids. The author describes the following effects produced on himself by a small quantity of hyoscyamia, which he took before one of his lectures. At the end of the lecture he felt a sensation of dryness in the mouth, throat, and larynx; the pulse fell

several beats, and the pupils of both eyes were a little dilated. He felt his head confused, and he had some vertigo, so that he reached home with some difficulty, and was obliged to use a stick in order to keep himself upright. All these symptoms increased remarkably at home; he was unable to read anything attentively, the pulse rose to 90 in three hours, although he was in a perfectly quiet sitting posture, and the natural pulse was 75. The dryness in the throat increased so much that he was unable to bring any morsel at dinner to the œsophagus without moistening it by abundant drinking. The sensation of dryness in the mouth, throat, and larynx, and the indisposition to mental exertion, continued till the period of sleeping; the sleep was quiet and deep. The next day he was quite well. The second and third experiments were performed upon a friend, who took on one occasion a two-thousandth part of a gramme, and upon another occasion, a five-thousandth part of a gramme, of hyoscyamia. The pulse was diminished in frequency in both cases, but in the second it fell at first from 79 to 18, and afterwards suddenly increased in frequency. In both experiments the head was confused, the secretion of saliva was diminished, the mouth and throat were very dry, and the power of swallowing in the second experiment much impaired; there was a feeling of feebleness, dilatation of the pupils; in the second experiment the taste and smell were diminished, headache supervened; and in both cases there was great propensity to sleep, which was quiet and deep.

As a medicinal agent, this alkaloid is serviceable when employed for the purpose of alleviating the irritation of coughing, and promoting sleep. In the latter circumstance it is inferior to morphia, and cannot easily be substituted for it when sleeplessness is caused by violent pain; but it has the advantage over morphia in promoting rather than retarding the evacuation of the bowels. The proper dose of hyoscyamia is one-sixtieth to one-twentieth of a grain, and is best administered as a powder with sugar. One-tenth of a grain is, according to Professor Schroff, too strong a dose. Hyoscyamia surpasses all other drugs in its local operation upon the iris. It acts more rapidly, and produces a more intense and continued dilatation of the pupil, when it is dropped into the eye, than any other medicine, and has besides the advantage of being soluble in water, forming a solution which is less exciting to the iris than daturia and atropia, which are soluble only in alcohol.

IX. *On the Use of Acetate of Lead in Yellow Fever.* By Dr. G. B. Wood. (American Journal of the Medical Sciences, October, 1856.)

Dr. Wood believes that the black vomit of yellow fever is not altogether attributable to the state of the blood, but somewhat also to the condition of the mucous membrane; and that an important indication in the treatment is to obviate the phlogosed condition of the membrane, and to induce a healthy action in its inflamed bloodvessels. These indications, he thinks, may be fulfilled by the acetate of lead, which is at the same time an energetic astringent and a decided sedative. He had tried it in three cases with decided success, but attention is necessary as to the period and circumstances of its administration. It should be commenced with at the earliest signs of the approach of the second stage, for after this it would probably be useless. It should be given in doses of two grains every two hours, without the accompaniment of any other substance that might tend to decompose it, and should be continued steadily until thirty-six grains have been taken.

X. *On the Treatment of Neuralgia by the Valerianate of Ammonia.* (L'Union Médicale, July 8th, 1856.)

Dr. Declat has warmly recommended the valerianate of ammonia in the treatment of neuralgia, and quotes the following very remarkable case in proof of its efficacy in that disease. A lady had been affected ever since six years of age with

a most severe facial neuralgia. The pain first appeared on the occasion of her cutting a wisdom tooth; the tooth was extracted, but without any relief of the neuralgia. All the ordinary means were tried in succession: internally, sulphate of quinine, opium, belladonna, sulphate of strychnia, iron, gold, &c.; externally, opium fomentations, blisters, morphia, chloroform, collodion, aconitine, &c. M. Jobert de Lamballe performed cauterization with a redhot iron in the course of the inferior maxillary nerve. This treatment diminished a little the acuteness of the pains, without making them disappear; and, although suffering less, the patient could neither eat nor speak. She was obliged for at least six months to have recourse to nutritive injections and tonic baths to support her health and her life. She was afterwards ordered to take twelve drops of a diluted Fowler's solution of arsenic three times a-day, and this treatment was followed by a little improvement; but the tongue became red, and the stomach painful, and on continuing the medicine at the urgent request of the patient, there were vomiting, diarrhœa, cramps in the stomach, and a return of the neuralgic pains. The arsenious course was then discontinued, and the valerianate of ammonia was ordered. On the 3rd of January, 1856, a teaspoonful taken in the evening rendered the night endurable; two spoonfuls the next day procured relief. On the 6th of January, the patient was able to go out and to converse; on the 19th she opened her mouth and began to eat. The dose of the remedy was successively raised to a dessert spoonful night and morning; the improvement was so great that the countenance assumed a totally different appearance, and the appetite returned. At last, on the 6th of May, the pains having completely ceased for several days, the use of the medicine was discontinued. From time to time some twinges of pain occurred, but each time the valerianate caused them to disappear, and Dr. Declat believes that there is no reason why the remedy should lose its efficacy in case of relapse.

In a subsequent communication,* Dr. Declat has stated that the valerianate of ammonia which he employs is a brown liquid, not very limpid, of a disagreeable taste, and smelling strongly of the peculiar odour of valerian; of this liquid he employs a teaspoonful for a dose in continued neuralgia and hysteria; but he gives two or even three teaspoonfuls in paroxysmal neuralgia, at the period of pain. Dr. Declat was first induced to try the curative effects of the valerianate of ammonia by observing the benefit which he experienced himself from its use when he was suffering from frequent headaches resulting from a severe attack of meningitis. It produced in himself the sedative effect of opium without the cerebral inconveniences which the latter drug always induced.

It should be mentioned, that the composition and properties of valerianate of ammonia are not yet accurately ascertained, and that different specimens obtained from a variety of sources are far from being uniform. It is therefore necessary that some standard preparation should be established before this remedy can be brought into general use.

XI. *On the Use of Chlorate of Potash*† *in Mercurial Stomatitis.* (L'Union Médicale, July 8th, 1856.)

At a meeting of the Société Médico-Pratique de Paris, M. Perrin described the good effects of the chlorate of potash in a case of mercurial stomatitis. In a lady, thirty years of age, attacked with acute inflammation of the uterus, mercurial frictions abundantly employed for several days on the abdomen, had produced very painful mercurial stomatitis, with impossibility of opening the mouth, of swallowing, of moving the tongue, and of speaking; and the whole of these symptoms complicated with an abundant and offensive salivation, which deprived the patient of sleep. Two grammes of chlorate of potash were prescribed in an

* L'Union Médicale, Aug. 30th, 1856.

† See British and Foreign Medico-Chirurgical Review, July, 1856, p. 244.

ordinary gum-potion, and given in spoonfuls every hour. The next day there was considerable improvement; the patient was able to open her mouth and to speak distinctly for the first time for two days. Four grammes instead of two were given in a second portion. After the use of this second potion, the improvement was so complete, that there was no occasion to continue the treatment any longer.

It should be mentioned that at a recent meeting of the Société Médico-Pratique de Paris, Dr. Otterbourg,* without denying the good effects produced by chlorate of potash, has expressed an opinion that the borate of soda possessed equal efficacy in stomatitis.

XII. *On the Use of Cod-liver Oil in some Cases of Dropsy.* By Dr. Filippo Sinibaldi. (Bulletino delle Scienze Mediche, December, 1855.)

Dr. Sinibaldi was led to prescribe the cod-liver oil in two cases of dropsical effusion, both of which terminated favourably. The first case was one of hydrothorax, in which the effusion was in the left cavity of the chest, pushing the heart over to the right side. The cod-liver oil was prescribed in the dose of two drachms twice a day after meals, the only other treatment being the use of tamarind pulp, and milk with a decoction of Iceland moss. The oil was increased in quantity to an ounce and upwards each day, and after about two weeks, an improvement was plainly manifested; the patient abandoned his lateral decumbency, the respiration was less difficult, some purulent matter was coughed up, the respiratory murmurs re-appeared with some mucous râles, percussion gave a less dull sound than formerly, and the heart removed from its abnormal situation. The oil was continued, and a nutritious diet was ordered, and the symptoms of disease gradually disappeared, and the patient left the hospital. The second case was that of a boy, aged eight, who was suffering from scrofulous disease, and after an attack of gastro-enteric fever, a fluctuation was perceived in the abdomen. The cod-liver oil was recommended, together with the administration of pills composed of soap, potash, and extract of cicuta. After four or five days of this treatment, the secretion of urine was increased, and the measurement of the abdomen showed a diminished quantity of liquid poured out into its cavity, and after a short time, all traces of œdema disappeared. The patient pursued the cod-liver oil treatment for some months, to which were added the iodide of iron and a meat diet, together with sea air and bathing. It was remarkable that in this case the urine was slightly discoloured and turbid, and gave out an ammoniacal odour. When exposed to heat, it presented a whitish, flocculent substance, analogous to the physical character of albumen; but the same effect was not produced by nitric acid.

XIII. *On the Subcutaneous Application of Medicinal Substances.* By Professor Kurzak. (Wochenblatt der Zeitschrift der Gesellschaft der Aerzte zu Wien, June 2nd, 1856.)

The inoculation of medicinal substances is performed very little by practical physicians. In Germany, Professor Langenbeck has tried a method somewhat modified by himself in a great number of medicines. The results are very interesting in their practical bearings. He calls his process the "hypodermic subcutaneous method."

The purpose of this proceeding is twofold: first, to bring a medicine immediately to bear upon a particular organ or part of the body, and secondly, by this application, to induce a derivation or revulsion. The absorption of the inoculated matter is facilitated by the very act of inoculation. But there are a number of substances, such as those which cannot be introduced in a fluid or semi-fluid state, the absorption of which is difficult and tedious; and these are mixed by Langenbeck with some exciting vehicle—such as croton oil, or tartar emetic, or both

* L'Union Médicale, October 21st, 1856.

together. In the *inoculation by needle*, the instrument must pierce deeper than in vaccination—namely, into the cellular tissue beneath the skin—and a much larger quantity of matter must be introduced. The inoculation-needle, therefore, presents a two-edged point, has the shape of a myrtle-leaf, deeply hollowed on one side in the form of a spoon. This two-edged point passes into a narrow, blunt stem or neck, which is so fastened by a hinge to a handle with double plates, that the needle can be pushed in like a lancet. The course of inoculation by needle is that the part becomes more or less red, and the skin warmer and harder. When the tissues are normal, there is formed a *passage* of the length of the needle introduced, and which feels hard and stringy, and is generally closed when the absorption into the deep part is completed. If the needle has been sunk from one point in several directions, there are several such *passages* formed, which often combine to form a small cavity. If the openings of the passages lie close together, an ulcer is formed by their union; it is then only necessary to introduce the medicine to be inoculated into this cavity; the substance is taken up in a short time into the canals passing from the ulcer, and generally in from four to six hours it has entirely disappeared. In the *inoculation by plaster*, the medicinal substance is laid upon a small skin-wound by means of a little thread of charpie, and kept in its place by a piece of sticking-plaster. The operation of this mode of subcutaneous inoculation is not so intense as in the needle inoculation.

A great variety of substances are enumerated by Langenbeck as having been employed by him in subcutaneous inoculation; and those were especially selected which are soluble in the cellular tissue or in the parenchyma of organs. For instance, strychnia was inoculated near the vertebral column for weakness of the spinal cord and paralysis; veratria for various skin diseases, as lepra, pityriasis, and scabies; quina for intermittent fever, inoculated into the breast or abdomen; digitalis for dropsy and palpitation, into the scrobiculus cordis; extract of squills as a diuretic, into the neighbourhood of the kidneys; cantharides, for incomplete paralysis of the lower extremities, into the lower part of the spine or the sacrum; cubebs and copaiba, for gonorrhœa, into the inguinal region, &c. &c.

XIV. *On the Chemical, Physiological, and Therapeutical Properties of Iodoform.* (L'Union Médicale, Sept. 4th, 1856.)

This body, discovered by Sénellas, presents itself in the solid form in the shape of glittering spangles, of a sulphur-yellow colour, friable, soft to the touch, of an aromatic persistent smell; it contains more than nine-tenths of its weight of iodine; its taste is sweet, and it has no corrosive property. Administered to dogs, it kills in a weaker dose than iodine, after having given rise to more or less marked depression, and rarely to vomiting. To the depression succeeds a period of excitement, convulsions, contractions, &c. Iodoform is quite destitute of any local irritant action, and does not occasion the slightest vascularity of the mucous membrane of the stomach or of the intestine. The therapeutical properties of iodoform, according to MM. Moretin and Humbert, are the following:—1. In consequence of the great quantity of iodine which it contains, it may be substituted for iodine, and the iodides in all the cases in which the latter agents are indicated. 2. The absorption of iodoform occurs with the greatest facility. 3. Iodoform, applied to therapeutics, possesses over other iodic medicines the advantage of not exciting any local irritation, or any of those symptoms which require the suspension of the latter preparations. 4. Besides the properties which are common to it with iodine, iodoform possesses special virtues: it calms the pain of certain neuralgic affections, and causes a kind of local and partial anæsthesia of the rectum when it is deposited in that part. 5. The doses to which it may be carried are 5, 10, 15, 25, 50 centigrammes *per diem*. 6. The principal diseases in which iodoform has been employed with advantage are, endemic goître, scrofulous disease, rachitis, syphilis, certain affections of the neck of the bladder or of the prostate, and some kinds of neuralgia. It may also be used in phthisis, in those cases in which iodine is employed with advantage.

XV. *On the Employment of the Silicate and Benzoate of Soda, combined with the Preparations of Aconite and Colchicum in the Treatment of Gout, Gravel, Chronic and Gouty Rheumatism, &c.* (L'Union Méd., Sept. 9th, 1856.)

In a memoir lately presented to the Académie Imperial of Paris, MM. Socquet and Bonjeau have proposed, in gouty and rheumatic affections, the employment of the silicate of soda and benzoate of soda. Silicate of soda facilitates the elimination of uric acid, and its influence may extend so far as to render the urine alkaline. This salt, moreover, by its tonic action upon the digestive functions and its diuretic properties, is said to be far superior to the carbonates of soda or potash, which are so constantly employed in the rectification of the uric acid diathesis. The benzoate of soda transforms uric acid into hippuric acid, the combinations of which are extremely soluble, while those of uric acid are hardly soluble at all. This medicine, in thus modifying the part of the acid which may have escaped the action of the silicate of soda, will thus contribute also to diminish its quantity. Colchicum will rapidly carry away, by the urinary passages, the remains of the uric acid which the blood may still contain. Aconite is used to act specially upon the painful part.

XVI. *On the Medical Properties of the Mineral Waters of Pougues.* (L'Union Méd., June 19th, 1856.)

The beneficial effects of the waters of Pougues were known to the Romans. There are two springs: the oldest, called St. Léger, is intended for drinking; and the second, discovered in 1833, serves for the administration of baths. The drinking water is cold and very gaseous; its temperature is 12° (Cent.?), and the specific gravity, 1003·12. Examined at the source, it appears to boil, an ebullition which is dependent on the disengagement of carbonic acid which exists in it in great quantity. When poured into a glass, it is limpid, inodorous, of a rather sharp taste, and alkaline. The water of Pougues belongs to the acidulated calcareous class; it contains a little iron. Its predominant bases are lime and magnesia; but as it contains also carbonate of soda, it is joined in this respect with the acidulous alkaline mineral waters, so designated because the predominating base is soda. The diseases in which the Pougues water is said to be most efficacious, are dyspepsia, chlorosis, especially when connected with alteration of the digestive functions; diseases of the liver and the spleen, particularly the engorgements of those organs; diabetes, which is said to have been often modified, sometimes cured, by its use; gravel and vesical catarrh, which are said to be peculiarly benefited; and gout and scrofula, for both of which the water has often been recommended with great advantage.

XVII. *On the Correct Appreciation of the Curative Powers of Ischl as a Watering-Place.* By Dr. JOSEPH POLAK. (Wochenblatt der Zeitschrift der Gesellschaft der Aerzte zu Wien, May, 1856.)

Chronic catarrh of the respiratory passages, simulating tuberculosis, is said by Dr. Polak to be arrested or removed by a residence of a few weeks. Persons suffering from pulmonary tubercle also visit the valley of Ischl for its remedial powers, and this they do before their autumnal and winter journeys to the south; and although the deleterious influence of the tuberculous process is often accelerated by mountain air, yet in certain cases, the patients have been known to regain their flesh, and to walk without fatigue; and this improvement has extended into the autumn and the winter, so that patients of this kind, when circumstances did not allow them to visit the south, have been exposed in a much less degree to the irritation of coughing and the pain of pleuritic seizures; and in women especially, the catamenial functions formerly interrupted have again assumed their regular course. Dr. Polak also thinks that exudations from the pleura and the

peritoneum resulting from inflammation of those membranes, are absorbed more rapidly than would otherwise be the case by the course of treatment pursued at Ischl. Swellings of the ovaries and the uterus, resulting from chronic inflammation, and hysterical forms of disease, are found to be remarkably benefited by a residence at this watering-place. Scrofulous diseases are benefited by the saline baths, especially as they are combined at Ischl with good air, and the opportunities afforded for all kinds of strengthening exercises. Dr. Polak opposes the general opinion, that Ischl ought only to be visited in the height of summer, for he states that at this period of the year the weather is often changeable and treacherous, while in spring and autumn, especially in the months of May, September, and October, the weather is more constantly fine, and exercises a most powerfully beneficial influence upon both body and mind.

XVIII. *On the Employment of Tartarized Antimony and Sulphate of Quinine in Acute Articular Rheumatism.* By M. Felix Barbeau. (L'Union Médicale, Sept. 4th, 1856.)

The idea of employing sulphate of quinine in large doses in acute articular rheumatism originated with M. Briquet in 1842; but although this treatment has succeeded in many cases, it has frequently failed. M. Barbeau attributes this failure to the neglect of due attention to the digestive organs in the treatment; and he suggests the employment of tartar emetic, with a view to causing an evacuation of the digestive passages, before commencing the administration of the sulphate of quinine. He adduces five cases of this mode of treatment, all of which terminated successfully. The tartarized antimony carrying away the noxious matters contained in the stomach, the sulphate of quinine is rapidly absorbed, and arrives in a short time at its destination; a fact which is indicated by vertigo, singing in the ears, and deafness. Care must be taken not to stop too quickly the administration of the sulphate of quinine, or even to lower the doses too suddenly, for in that case the fever will return immediately with great intensity. It is necessary to continue the same dose until the pulse returns to its habitual frequency. The use of sulphate of quinine is attended with some inconvenience: it sometimes happens that it cannot be borne, and that it excites vomiting. In the first case recorded by M. Barbeau, the patient was improving, but his dose of quinine was omitted by accident, and there was an aggravation of all the general and local symptoms; but on resuming the medicine, the symptoms again subsided, and the patient rapidly recovered.

QUARTERLY REPORT ON SURGERY.*

By John Chatto, Esq., M.R.C.S.E., London.

I. *On the Duration of the Incubation of Syphilis.* By Professor Sigmund. (Wien Wochenschrift, No. 18.)

Researches made in great numbers, under most varied circumstances, in all parts of the world, have pretty exactly determined the *shortest* period within which secondary symptoms first plainly appear after the production of the chancre. This is found to be the sixth week, or soon after, an earlier period being very rare, and a much later very uncommon. But when we wish to ascertain the *longest* period within which such symptoms may become developed, the most opposite views prevail; the so-called *latency* lasting, according to some, for a few months, and to others, for some years. The solution of a problem as difficult as it is important, can only be obtained at the hands of specialists dealing with a series

* Press of matter compels us to postpone the Quarterly Report on Medicine.

of varied and long-observed cases. The differences of opinion upon the point are due to defective examination and observation of the patient, erroneous computation, varying ideas as to which constitute secondary symptoms, and the difficulty with which such symptoms are in some cases recognised.

As to defective examination of the patient, daily experience shows that eruptions, glandular swellings, excoriation of fauces, &c., are either overlooked or are attributed to other causes. A careful examination of the lymphatic glands is of the rarest occurrence, unless the patient complain of pain. But acutely painful and rapidly suppurating glands are of far less significancy, in relation to secondary syphilis, than the slowly enlarging, hard, non-suppurating, nearly painless glands. Even an accurate examination of the chancre does not always take place, providing that this has but cicatrized over—this being, unfortunately, in the idea of most practitioners and patients, identical with a cure; and yet the condition of this has an essential signification. Not the slightest value is to be placed upon the assertions of the patient, careful and repeated examination alone serving as a guide.

Although specialists may agree as to the signs of secondary syphilis, this is far from being the case with practitioners less familiar with the disease. To the first series of symptoms belong induration of the chancre, and indurated, almost painless, enlargement of the glands nearest to the chancre; enlargement of the other glands accessible to the touch, as in the axilla, neck, &c., forming a continuation of the first series of symptoms. Papulæ on the skin and mucous membranes, especially near the genitals and anus; spots on the skin, and sharply circumscribed redness; swelling and superficial ulceration of the fauces, are soon added to the first symptoms. Such appearances are found in pretty constant succession within the first three months. Examining 1473 cases that have occurred to the author during the last five years, with reference to the period of incubation, 293 have been selected, either because copulation had occurred only once or only after a very long interval had elapsed, and because the characteristic chancre, still present, had undergone no essentially influential treatment. In these 293 cases, the lymphatic glands were affected in all, the chancre was indurated in 261, the fauces were affected in 248, spots on the skin existed in 204, and papulæ, pustules, or condylomata in 134, besides undergoing various combinations with each other, that we need not transcribe. The following is the scheme of the period of the respective appearances of these symptoms:—

	In the 4th	5th	6th	7th	8th	9th	10th	11th	12th week
Enlarged glands	31	44	56	71	46	20	13	9	— times.
Spots of skin	—	—	2	41	68	45	22	11	2 „
Papulæ and pustules	—	—	—	3	10	11	24	27	31 „
Affection of fauces	—	—	—	7	22	34	41	41	42 „

	On the 9th	10th	14th	17th	19th	21st day.
Induration of chancre	71	84	76	15	12	3 times.

Secondary symptoms may, however, exist, without being thus distinctly exhibited, and these difficult cases are only possible of recognition when we have known the patient, and carefully examined him prior to his illness, or from the beginning of the affection. They consist in changes in the skin, which becomes of a pale, white, fawn, yellow, or brownish colour; it is wrinkled, dry, harsh, rough, and hard, its soft and elastic turgor having disappeared. The muscles lose their hardness, elasticity, and energy. Nutrition is defective, and there is often rapid emaciation; various kinds of disturbance of the digestive organs being present. Pains are present in various parts of the body, especially in the head, neck, joints, and muscles; sleep is disturbed; and the spirits are dull and devoid of tone. All these symptoms may be due to other causes, but it is the office of the circumspect practitioner, in these less marked pictures of disease, to form his diagnosis by combining and carefully weighing all appearances. Although such cases are rare, yet is their occurrence not to be denied; and

they are of importance, because, at a later period, and sometimes very late, other series of secondary and tertiary appearances may be developed, and then the interval that has elapsed is reckoned as part of the incubation-period. In these cases the author has almost always found the glands affected, though this may be to a slight extent. It is, in fact, in cases in which the forms are not very tangible, but much oftener because the patient has not been subjected to exact and well-timed examination, that instances of prolonged incubation-period of syphilis have been said to occur. The existence of such instances are ignored by the author; and he lays down his position, that when a chancre has healed, and manifests no induration within the first three months, and when none of the already-mentioned appearances are manifested, the patient has no cause to fear further syphilitic symptoms, unless he have, in the mean time, exposed himself to a new infection.

II. *On the Valves of Abscesses and Fistulæ.* By Professor ROSER. (Vierordt's Archiv, 1856, pp. 349—354.)

In an abscess that has spontaneously opened, we often may observe a valvular mechanism, permitting the pus to flow out, but not allowing the entrance of air. This may be the case, not only with abscesses opening upon the skin, but those discharging upon internal membranes. Thus, we sometimes find an empyema emptying itself through the bronchi, the most careful examination failing to detect the admission of air into the cavity of the abscess. The same thing occurs in several abscesses within the abdomen. Collections of matter may be discharged through the intestinal canal, the intestinal gases or fæcal matters not reaching the abscess. So also in respect to the rectum, the bladder, the urethra, and the trachea. Indubitably, in many of these cases, a valvular mechanism must be supposed to be present; and to this, and its prevention of the putrefactive decomposition of the contents of the abscess, many a patient has owed his life.

But all valves of abscesses are not thus useful, some being injurious in their operation, as when they obstruct the issue of the pus, and prevent the complete emptying of the abscess. Such abscesses keep filling again and again. Under the influence of forcible distension or acute suppuration, the mouth of the abscess becomes from time to time widened, and the valves are pressed aside or torn through; but they are soon reproduced, and the obstruction to the flow again occurs, so that such collections may last for years. The valvular condition of such abscesses can be demonstrated; for the pus cannot be pressed out, although the opening may be large. But if a catheter or other tube be introduced, or the opening be enlarged by a knife, a considerable quantity of pus is often suddenly discharged. These valves may be often temporarily displaced by a sound; and by its daily introduction, many abscesses of this description may be healed; but in more obstinate cases, the introduction of tubelets or repeated incisions are required.

As a general rule, it is not in subcutaneous abscesses that we find this valvular mechanism, but in the more important and deep-lying collections—as in empyema; deep cervical, post-mammary, or axillary abscesses; and in those of the abdomen and pelvis; and in periosteal and perineal collections. The valve may not always be found near the orifice, and the deeper it is placed the less are we disposed to pursue it with the knife. In such cases we must endeavour to widen and maintain the opening by means of metallic or caoutchouc tubelets, or sponge tents. In many places, as in the deep parts of the thigh, the neck, and the pelvis, it is preferable to produce forcible dilatation with a forceps to penetrating too far with the knife. Professor Roser has often had recourse to dilatation of this kind in deep-seated acute or chronic abscess, in the removal of osseous sequestra, and in operations for hernia.

Three kinds of these valves may be distinguished:—1. The ***obliquely-placed valves*** are the most common, and they may be called from their describer, Abernethy's

valves. As physiological examples of such, the entrance of the ureter into the bladder, and of the parotid ducts into the mouth, may be adduced. In the lip-form fistulæ a similar valvular mechanism is brought into play, an example of which occurred in Dr. Beaumont's case of fistulous opening into the stomach. In a case of lip-form vesico-vaginal fistula which occurred to the author, a catheter could be easily passed along; but if air or water were injected into the rectum, no portion, owing to the oblique direction of the valve, entered the vagina. 2. Among the obliquely placed valves, the *wart-like* may be included. The mouth of the abscess is surrounded by spongy warts, from the midst of which the pus issues, while the admission of air is prevented by the warts pressing inwards so as to close the orifice. Such openings are not infrequently observed at the surface; and Professor Roser believes that, in many intestinal perforations, a similar mechanism prevails. 3. The *pad-like* valve, which is analogous to the valve of an air-pump, and an example of which is found in the valve of the prostate. It is this form which especially opposes the exit of pus, and calls for surgical interference.

III. *On the most eligible Spot for the Performance of Amputation of the Leg.* (Gazette des Hôpitaux, Nos. 116, 117, 120, 126, 129, and 131.)

A prolonged discussion upon this subject has recently taken place at the Société de Chirurgie. M. Larrey took occasion to observe, that the soldiers who have of late arrived from the Crimea, having had amputation performed at the middle third or lower part of the leg, were in so bad a condition as to lead to the conclusion, that amputation at the place of election must in the end prevail. The difficulty in employing artificial limbs is so great, and the accidents which result are so numerous, that the patients at last find themselves obliged to resort to the wooden leg. M. Chassaignac, believing our first duty to be the preservation of life, thinks we should never resort to the place of election when we can perform supra-malleolar amputation. M. Verneuil stated that he had paid much attention to the ulterior effects of amputations, and he thinks that supra-malleolar amputation has been too exclusively recommended. There can be no doubt but that the immediate mortality is far less than after the old mode; but we should also take into account the amount of ulterior benefit derivable by the patient. Startling as the assertion may seem, he thinks that in certain cases it is better to run the chance of a greater mortality, than to perform an operation that may prove useless and require repetition. Supra-malleolar amputation is much oftener followed by conicity and other defective states of the stump, than is amputation high up; while osteitis, caries, or necrosis of the bones of the leg, is a more frequent result. This last usually has occurred when the operation has been performed for disease of the tibio-tarsal joint, the osteitis of the bone having spread from the disease of the joint. The first results of the operation are deceptive—for it has an antiphlogistic effect—and for some months the patient may seem cured. But later, either spontaneously or from slight causes, the osteitis is reproduced, and may necessitate secondary amputation. Therefore, whenever amputation is performed for disease of this joint, it should be practised at the upper third. But in traumatic affections, and in disease of the bones of the foot, in which those of the leg but little participate, the supra-malleolar operation is preferable.

M. Guersant has found, in operating upon children, that the mortality is the same in both localities; but from his patients having in after-life to provide for their living, and finding difficulty in getting artificial limbs, he prefers operating at the place of election. M. Huguier dwelt upon the relative safety of the supra-malleolar operation, having lost only one patient in 14 cases; but he admits that the predilection for this operation which his success imparted to him, has undergone considerable modification on observing its ultimate consequences. These never follow when the operation is performed for traumatic lesions, and he does not recommend it in the case of white swelling. M. Broca admits that many

patients who have undergone supra-malleolar amputation, have suffered severe accidents from want of a suitable prothetic apparatus; and great is the inconvenience produced by the long stump when a wooden leg is resorted to. Still these effects are as nothing when compared with the greater safety of the operation; and while it is admitted that $\frac{5}{6}$ths of these patients recover, more than half of those die who are operated upon at the place of election. Even in those cases when necrosis demands another operation, secondary amputation is less fatal than primary. As to the question of the ultimate effects of the two operations upon the stumps, after amputation at the place of election, the patient rests upon his knee, which gives him a firm support, but he is deprived of the power of flexion and extension of the joint. After the supra-malleolar amputation, the artificial limb is supported at the ischium, and a hinge-joint allows of such movements at the knee, that it is quite surprising how perfect a substitute the apparatus becomes. It is true that the poor only obtain ill-made apparatus, which frequently get out of repair, and often ultimately produce irritation and ulceration of the stump. Still it is the duty of the surgeon to perform that operation which saves most lives, and leave the supplying these defects to others.

M. Robert observed that if the relative amount of mortality were to decide the question, there could be no doubt about the preference. In children, however, amputation at the place of election is preferable, for the mortality is not greater, while there is difficulty in fitting a prothetic apparatus and necessity of changing it. Even in the adult, the question of preference is doubtful, when the occupations of the patient are laborious, for he then often forsakes the artificial limb for the greater solidity afforded by the wooden leg. Then, again, the nature of the lesion should exert great influence upon our decision. When it affects the foot, but not the joint, the supra-malleolar operation is preferable, but it should not be had recourse to in the case of white swelling of the joint. M. Giraldès thought that the instances of the soldiers coming from the Crimea, given by Larrey, were hardly fair examples of the effects of supra-malleolar operations, inasmuch as such patients had suffered much in the ambulances, and in shifting from hospital to hospital. He believes that some of the evil results are due to the application of apparatus prior to complete cicatrization. M. Hutin stated that during the eleven years he had been at the Invalides, he had had more than two hundred soldiers under his care who had undergone amputation. In the great majority it had been performed at the place of election, or above this, and in not a single case had he observed any rupture or ulceration of the cicatrix. Among those patients, however, in whom it had been performed at the lower third, these were common. The fusiform disposition of these stumps, the almost constant presence of ulceration, and the inconvenience produced by the constriction of artificial limbs, induce the patients to reject these in favour of the wooden leg. With this, the large projection of the stump behind is most inconvenient, and gives rise to the production of great irritation. During winter, the stump becomes cold, violaceous, tense, and painful, while ulceration of the delicate and unsupported cicatrix is almost constant.

IV. *On Orchitis.* By M. Velpeau. (Gaz. des Hôpitaux, No. 122.)

In the course of his annual clinical review at La Charité, M. Velpeau made some interesting observations upon the cases of orchitis. These were 50 in number, 48 being acute and 2 chronic. It was remarkable that 24 occurred on the right and 24 on the left side, two cases being double—one of these being an example of tubercular disease. M. Velpeau observed that examples of tubercular testis should teach us the caution necessary in laying down absolute laws in pathology. Louis has laid down such a law in stating that when tubercles are found in any other organ, they will also be found in the lung; but the testis offers numerous excep-

tions to this, which it is necessary to bear in mind, lest our prognosis be needlessly unfavourable.

Of the 48 acute cases, 3 were parotidean, 2 were due to masturbation, 6 occurred without appreciable cause, and 37 arose from gonorrhœa. The variety of orchitis due to mumps, of which there were three examples, should be distinguished from the others, as it has neither the same duration, mode of progress, or appearance. The epididymis is moderately swollen, the testis is increased in size, and the scrotum is slightly erysipelatous, while there is generally no fluid in the tunica vaginalis. This form is rapidly developed, reaches its height almost at once, and then decreases spontaneously, resolution soon being completed. It is evidently quite a special kind of inflammation.

In several cases masturbation was suspected, and in two was ascertained to be the cause; and it is easy to see how orchitis may arise from irritation induced at the lower part of the urethra by this practice. It is, however, only of late years, after close interrogation of the patients, that M. Velpeau has admitted this as a cause of orchitis. It is a variety also requiring to be studied apart. There is less swelling of the epididymis, and little or no fluid. If the cause ceases, resolution takes place in three or four days.

Six of these cases are said to have occurred without appreciable cause,—that is, independently of all inflammation or irritation of the urethra. The patients often attribute the occurrence to a strain, but the data furnished by anatomy have led to the denial of the influence of this cause, inasmuch as compression of the cord cannot be produced by the external ring. This doctrine has prevailed since the time of Winslow, but then the external ring only was taken into account. Since then it has been shown that a bundle of fibres extends from the external edge of the aponeurosis of the rectus to the crest of the ileum. These form an arch with its concavity upwards, upon which the cord lies, making a more or less acute angle at the internal orifice of the inguinal canal. It is the compression exerted by this fibrous arch during straining that may become an occasional cause of orchitis, when it has been carried far enough to notably impede the circulation through the cord.

Of the thirty-seven cases of gonorrhœal orchitis, in seven or eight there was no notable quantity of fluid in the tunica vaginalis; and in employing punctures in orchitis, there never flows out a quantity of fluid equivalent to the volume of the tumour. We may always observe swelling of the epididymis or of the testis, or of both. The fact of simultaneous swelling of the epididymis and of the testis shows the impropriety of the term epididymitis that has been applied to orchitis. It is, indeed, often difficult to determine the presence of fluid when there is swelling of the testis, or even when the testis itself is healthy. The testis gives to the finger, in fact, a sense of fluctuation. But if we grasp the scrotum, so as to cause the tumour to project forwards, if there is even but a thin layer of fluid, we find it presenting a non-resisting plane to the finger, which, giving way, allows us to come upon a more resistant plane, in which we still perceive fluctuation. This last is the testicle; but to distinguish slight accumulations, it requires that the finger should be well exercised. The vas deferens is affected in the majority of cases, being swollen and painful; and this is of importance, for such a condition of the canal implies a longer duration of the orchitis. The testicle may indeed be compared to an inflamed gland, and just as sometimes we do not perceive the inflamed absorbent vessel, so here there may be an absence of swelling of the vas deferens. Swelling of the epididymis also implies a longer duration of the affection; and it may be stated that this will be less in proportion as the testis is more affected than the epididymis and the vas deferens.

The mean duration was in these cases sixteen days; forty-six of the patients were cured, two were not so when they left, and one of these afterwards died. In this case, the orchitis was not very severe, and succeeded to a mild gonorrhœa, contracted by a young man having hypospadias. He died of peritonitis; and on examination, all the seminal passages were found to be the seat of blennorrhagia.

The vesiculæ seminales were in a state of suppuration, and the peritonitis had originated at the recto-vesical cul-de-sac. This is a rare case; but, as M. Velpeau has long since remarked, inflammation of the vesiculæ seminales is by no means a rare affection after gonorrhœa. The treatment of these patients has consisted in the employment of rest, cold, suspensories, mercurial inunction, and either single or multiple punctures with a lancet, abstaining from leeches. Punctures, by giving issue to the fluid, give great relief to the patient, certainly abridges the duration of the disease, and is exempt from inconvenience. In appreciating various modes of treatment, we must never lose sight of the varieties of the affection, for these will explain much of the success said to follow some of the modes proposed.

V. *The Hæmostatic Douche.* By M. Gaillard. (L'Union Médicale, No. 127, 1856.)

M. Gaillard gives this name to a contrivance he has often had recourse to during the performance of operations, especially upon arteries, which, without giving rise to an amount of bleeding that can be termed hæmorrhage, yet induce it in sufficient quantity to obscure the steps of the operation, imparting the same colouring to all the cut parts. The more the coagula are wiped away, the more does the tomentous cellular tissue become infiltrated with blood, forming a kind of pseudo-membrane, beneath which all distinctions of muscle, aponeurosis, and vessels disappear. The means consists in placing a vessel full of cold water upon an elevation near the patient, and connecting with it a flexible syphon, terminated by a straight canula. An assistant, watching the steps of the operation, guides a continuous stream of water over the surfaces as they are divided, regulating the force of this by means of a small cock. The surface of the incisions thus made under water are kept quite free from blood, and the distinctive colour of the various parts is easily observed.

VI. *On Auscultation of the Ear.* By M. Gendrin. (Comptes Rendus, tome xliii. No. 9.)

M. Gendrin, believing the diagnosis of the diseases of the deep-seated portions of the ear may be facilitated by the employment of auscultation, communicates the results of six years' examination of the subject. The auscultation may be either mediate, or immediate, and the patients' nares should be closed.

In the physiological condition every expiration produces in the tympanum a deep, gentle, distant *bruit de souffle*, which passes away before the end of the expiratory movement. If the *membrana tympani* is perforated, this sound becomes acute, dry, sometimes even sibilant, and is more prolonged. When the Eustachian tube is narrowed it becomes intermittent, consisting of several successive *souffles*, which are usually accompanied with bullar crepitation due to mucosities. Crepitation may be heard when there is caries of the internal ear, or when there is a collection of matter on the middle ear, or in the mastoid cells, in communication with the tympanum and the open tube; but here the crepitations are deep and moist. Coughing renders these abnormal sounds shorter and more clear, so that they are more easily recognised.

Inspiration, in a sound ear, does not give rise to any perceptible sonorous vibrations; but if the membrane is pierced, the Eustachian tube remaining pervious, a very sharp sibilant *souffle*, mingled with moist crepitation, is heard, the patient himself often being conscious of the sound.

The voice, heard in the ear, appears deeper and slightly vibrating, and is interrupted by frequent and sudden intermissions. When the tube is narrowed, or the tympanum is filled with mucosities, by pus or by a central exostosis of the petrous bone, it degenerates into a confused and inarticulate murmur. It is not heard when the tube is obstructed; and it becomes whistling, and is accompanied

by crepitating bullæ, when the membrane is ruptured. In the normal state, the labial hissing is transmitted by the ear like a distant acute *sibilant souffle*. It is much weakened or almost silent when the tube is narrowed, and is not heard at all when this is obstructed. When the membrane is destroyed, the tube remaining free, the hissing becomes very acute, and is so near that the patient seems to be whistling in one's ear. In most cases the abnormal sounds may be verified by a comparative auscultation of the ears, as it is rare to find the same degree of the same lesion in the two ears.

VII. *On Galvanism in Opacities of the Cornea and Nervous Palpebral Palpitation.* By Dr. A. Quadri. (Annales d'Oculistique, vol. xxxvi. p. 41.)

Dr. Quadri is of opinion that one reason why electricity is not oftener serviceable, arises from its employment being reserved for those cases in which all other means have failed, cases of confirmed paralysis due to changes in the nervous substance, over which it exerts no control. If it were resorted to for the treatment of slight neuroses, success would be more prompt and easy; and patients might not require dosing with medicines which are as often dangerous as useful. On the present occasion, Dr. Quadri first relates the results of the application of galvanism by means of Bunsen's pile in two cases of marked albugo. Very few applications, each continued for five minutes, effected marked amelioration. In old and very thick leucoma he has found electricity of hardly any avail.

While engaged in trying the remedy in amaurosis, for which disease it seems to have been of very little avail, he observed that it exerted remarkable power over the nictitation which so often accompanies amaurosis. It then struck him that it might prove of utility in the nervous affections of the muscles of the eye, nerves to which a direct application could be made, impossible in the case of the optic nerve. Four cases are given in which the remedy seems to have been of very considerable avail. In the first, persistent convulsive movements of the eyelid, accompanied by great photophobia, had resisted other means. In the second, involuntary contractions due to bad acquired habits, occurring in a girl ten years of age, were relieved. The third was another example of convulsive motion of the eyelid, with photophobia; and the fourth was a case of ptosis.

VIII. *On Vicious Cicatrices.* (Gazette des Hôpitaux, No. 116, 1856.)

At a recent discussion at the Société de Chirurgie, some interesting observations were made upon this subject. M. Chassaignac, speaking of the cicatrices from burns, called attention to the importance of operating for these when occurring in children at as early an age as possible, inasmuch as the parts influenced by cicatricial bridles fall into a state of atrophy, or rather, suffer from an arrest of development. Thus, in a case in which the little finger had been kept flexed by a bridle, the finger, after rectification, never attained to more than half the size of the other. What is very remarkable in these cases is, that the joints remain uninjured, in spite of their long disuse; but nevertheless the motions of the limb cannot be at once re-established after the division of the bridle, for the insufficient development of the soft parts beneath the cicatrix prevents the necessary extension being made without laceration. In the adult, in whom development was completed at the time of the accident, such complication is not to be feared. The rule for these operations, therefore, is, that while in the adult they may be deferred without inconvenience, in children they should be performed as early as possible. M. Guersant also had found, after complete division of cicatrices at the bend of the arm in children, great resistance made to extension; but he never attempts to use force to overcome it. He relates a case occurring in an adult, who was twenty years old when burned, and in whom the limb offered no resistance to extension made six years after. M. Verneuil observed, that it is proper to wait

before operating until cicatrisation is quite completed, a year sufficing to confer all its properties on a cicatrix; but we must not overlook the consecutive lesions that are developed with time. M. Desormeaux related an interesting case of a young girl of about sixteen, in whom the knee was powerfully flexed and the foot extended, in consequence of a bridle formed after a burn occurring in infancy, and which extended from the thigh to the heel. By division of this, and of several of the flexors, the position of the limb was rectified; but the limb was atrophied, small, and short, great lameness resulting. Restored to its normal position, however, it rapidly increased in size, so that in two or three years it had become almost as large as the other, and had so increased in length that lameness no longer existed. M. Larrey noticed the great difficulty military surgeons have in deciding whether the amount of immovability in certain contracted limbs is wholly or in part simulated, and he recommends the following plan for determining this. The two limbs are placed side by side, and gentle movements are imparted to them. Presently, and all of a sudden, this movement is rendered rough. If the affection be simulated, the patient being unable to completely separate in his mind the movements of the two sides, the two limbs yield or resist together, it being sometimes the sound side that resists the most.

IX. *On the Treatment of Bubo.* By M. Broca. (Bulletin de Thérapeutique, tome li. p. 208.)

M. Broca observes that a bubo undergoes two stages of development, during the first of which the inflammatory engorgement is confined to the gland itself, this containing a small central cavity filled with semi-fluid pus. In the second stage, suppurative inflammation is propagated to the surrounding cellular tissue; and it is by such extension that the ravages of bubo are produced. The object of the proposed means of treatment is to prevent the production of this secondary abscess, by attacking the bubo during its first stage, and evacuating the pus before this has extended beyond the limits of the gland itself.

M. Broca prefixes some observations upon the diagnosis of the form of bubo that should be so treated, these being based upon Ricord's doctrines. Such bubos are indurated, rounded glands, the skin over which is not discoloured, and they have very much the appearance of the indolent bubo met with in the first stage of constitutional syphilis, but which, never suppurating, requires no local treatment. This indolent, *constitutional* bubo is in fact one of the first symptoms of secondary syphilis which follows indurated chancre, and is amenable to mercurial treatment. The *local* suppurating bubo never appears but in glands which are in direct communication with the part that is the seat of chancre, which chancre is never indurated, and never gives rise to constitutional syphilis. It is amenable only to local treatment, and the existence of a glandular abscess is sufficient to conclude that the syphilis is local, and that mercury is inexpedient. When this *local* bubo has reached its stage of complete development, there is therefore no difficulty in its diagnosis; but at first, prior to the propagation of the suppurative inflammation to the cellular tissue, it may be confounded with constitutional bubo. But, as has been stated, this last almost constantly arises from indurated chancre, which is never the case with the local bubo. The constitutional exists on both sides, the local is very often unilateral. The latter is never accompanied by symptoms of constitutional syphilis, while in the former there are always more or less evident signs of a general infection, which gives rise to other analogous glandular engorgements, and especially at the postero-superior cervical region. The tumour in constitutional bubo is quite indolent, while the other is always more or less painful, especially upon pressure. In the former there are generally a considerable number of glands engorged, which are scattered over the whole extent of the bend of the groin; while in the latter, but two or three glands, placed close to each-other, and often only one, are affected. In constitutional bubo the tumour is very hard and

entirely solid; but in local bubo it is somewhat less hard, and imparts a sense of fluctuation similar to that furnished by a small cyst with very thick walls. This fluctuation alike differs from that of an ordinary abscess, and from the resistance of solid tumours. It is due to the semi-fluid purulent matter contained in the centre of the gland.

The accurate diagnosis is of importance, as the treatment recommended is applicable only to the local bubo. When the gland has acquired the size of a small hazel nut, it should be firmly fixed by two fingers of the left hand, and a bistoury plunged into its centre. Without letting go of it, the bistoury should be removed, and a grooved director passed in. On employing strong lateral pressure, a small quantity of semi-fluid, ill-elaborated pus is forced along the groove; and the pressure must be continued until the blood comes, so as to secure the entire discharge of this pus. It is rather a painful procedure, and must be repeated on each affected gland. The tumour becomes a little reduced in size, but next day it has somewhat enlarged again, and the small quantity of pus that has again formed must be discharged by passing in the director and using pressure. This must be done every day until either suppuration ceases, or a small fistulous opening has become established for the discharge. In some of his cases, M. Broca has injected tincture of iodine by means of a small syringe, and he thinks this may exert some effect in neutralizing the virulent properties of the pus when this is inoculable. At present but 9 cases have been treated by this new mode, no ill effect having resulted in any of them; while extension of suppuration to the cellular tissue, with the consequent ulceration, detachment of skin, &c., has been avoided. In 5 out of the 9 cases, less than a week sufficed for a cure; the other cases requiring twelve, thirteen, thirty-seven, and fifty days; a small fistulous opening alone remaining during that period, in place of the large purulent collection usually observed. This mode of treatment, therefore, even when it does not abridge the duration of the bubo, materially restrains its extension.

Since the above paper appeared, M. Gély, surgeon of the Hôtel-Dieu, Nantes, has published* an account of some observations he made upon the subject in 1852-3. He states that he has derived great advantage from making punctures with a lancet at an early period, sometimes as soon as the third or fourth day. He introduces no conductor, and employs no pressure, but makes a puncture large and deep enough to allow of a free escape of the pus; and applies a tepid cataplasm if there is much inflammation.

QUARTERLY REPORT ON MIDWIFERY.

By Robert Barnes, M.D. (Lond.)

LETTSOMIAN LECTURER ON MIDWIFERY, ETC. ETC.

I. Diseases of the Generative Organs.

1. *Case of Vesico-Uterine Fistulæ cured by Operation.* By M. Jobert (de Lamballe). (L'Union Méd., Nov. 22, 1856.)
2. *Case of Rupture of an Ovarian Cyst, and Absorption of the Fluid.* By Dr. Gautier. (L'Union Méd., Nov. 25, 1856.)
3. *Two Cases serving to illustrate the Therapeutical History of Ovarian Cysts.* By Dr. Th. Herpin. (L'Union Méd., Nov. 8 and 11, 1856.)

1. M. Jobert's case of vesico-uterine fistula is remarkable. A woman, aged twenty-seven, entered the Hôtel-Dieu on the 4th June, 1856. She was in tolerable health. Menstruation had appeared at the age of twenty. She bore her first child at twenty-two; nothing remarkable followed. She married at twenty-four, and conceived soon after. Six weeks after marriage, she suffered from inflammation of the lower abdomen; abortion of a two-months' ovum followed. Eight days

* Moniteur des Hôpitaux, Nos. 133, 136, 138.

after this abortion, she first perceived that her urine came away involuntarily. Menstruation was suppressed. A curved sound introduced through the os uteri met a silver catheter passed into the bladder by the urethra.

M. Jobert performed the following operation:—With toothed forceps and bistoury the borders of the os uteri were bared, and freshened so as to make two bleeding surfaces; then, by means of curved needles borne on handles, and carrying waxed threads, three points of suture were applied, so as to bring together the lips of the os uteri, and completely to obliterate the opening. A catheter was placed in the urethra. The result was cicatrization of the os uteri and closure of the uterine communication with the vagina. Henceforth the incontinence of urine was removed: the urine came by the urethra; and the catamenial discharge flowed through the same canal. The operator, considering it impossible to close the vesico-uterine fistula by direct proceedings, had resorted to the operation described, which had the effect of diverting the menstrual flow from its usual course into the bladder and urethra, at the same time that the urine was made to take the same course. He conceives that the patient was relieved of a loathsome affection at a moderate cost, by the loss of the reproductive function.

2. A single woman, bearing a large ovarian cyst which had been developing for some years, followed, under Dr. Gautier, a long course of purgative, diuretic, and ferruginous medicines. One day the patient was carrying a full kitchen boiler, when she tripped against a chair, and fell with her left side on the edge of the boiler. She fainted: great pain followed; but she was not conscious of any sensation of laceration. On external palpation, however, the resistance of the tumour had vanished; fluctuation was freely discerned, and the liquid always fell to the lowest point on moving the patient. The liquid was gradually absorbed; and there remained nothing of the tumour but a hard nucleus.

3. Dr. Herpin relates two instructive cases of ovarian dropsy, of which the following is a summary:—1. On the 27th June, 1838, a woman, aged twenty-nine, had been married fifteen months; her husband stated that she bore at that time a small tumour over the right groin. During the last month, this tumour increased rapidly, and began to disturb the intestinal functions. On the 3rd August, pregnancy, which had not existed at first examination, was suspected; the size of the abdomen had greatly increased. On the 4th, the patient was seized with colic and diarrhœa, followed by intense abdominal pain; other attacks came on at intervals of a few hours, and caused collapse. On external examination, it was discovered that the elastic resistance of the tumour was gone, that the tumour had lost much of its volume and consistency. Symptoms of peritonitis came on. This was subdued by leeching, mercurial inunction, and opiates. The cyst grew again, so that at the end of the term of gestation, it had reacquired the size it had before rupture. Labour came on at the period prognosticated, but proceeding slowly, M. Mayor, under whose care she had come, fearing some accident from the violent efforts made by the patient, applied the forceps, and brought forth a living child. She made a good puerperal recovery. The characters of the ovarian cyst became clearly defined. Four years later, the cyst had enlarged considerably, and caused great functional disturbance. Tapping was resorted to repeatedly. After the third, it was perceived that another tumour was springing up to the right of the ovarian cyst; the patient was again pregnant. Several other punctures were made, but the patient sank, with all the signs of advanced phthisis, at the end of seventeen months from the first tapping.

Case 2.—Cure of an ovarian cyst by absorption, through sudorific treatment and purgatives.

On the 14th May, 1844, a woman, aged forty-five, still menstruating, having had two children, had perceived for some months a tumour, recognised to be an ovarian cyst. M. Herpin prescribed, for every two days out of three, a packing, after the hydropathic fashion, in blankets, and every two hours, until sweating,

Dover's powder. Profuse sweating was caused; the blankets were dripping. After fifteen days of this treatment, the tumour had sensibly diminished. After a week's repose a purgative course was adopted. Anderson's pills were given every two or three days. Her health improved; the tumour decreased. Three years afterwards, the tumour could not be felt, and after six years it was ascertained that there was no relapse.

II. Diseases of Pregnancy.

A Case of Vomiting during Pregnancy. By Dr. Turnbull. (Australian Medical Journal, No. 2. April, 1856.)

The case of vomiting during pregnancy related by Dr. Turnbull of Victoria, is interesting on account of the clearness of its pathology. On the 22nd of September, 1855, he saw Mrs. S., who was suffering from persistent vomiting, with quick weak pulse; no pain on pressure over the epigastric region; she was between seven and eight months' pregnant. Hydrocyanic acid; sinapisms to the abdomen; mistura creosoti (Ph. Ed.); chloroform, at first in doses of five minims, half-hourly, afterwards increased; were successively tried and totally failed. The patient was unable to retain even a teaspoonful of cold water. On the next day vomiting persisted, patient almost pulseless; premature labour determined on; os uteri found partially dilated, feet presenting. The membranes being ruptured, the vomiting ceased almost immediately. A living female child was extracted. Placenta removed easily. The vomiting returned five or six hours after delivery. Half a grain of opium and two and a half grains of bismuth were ordered frequently. A small clot was discharged, and on examination a substance was felt protruding through the os; this removed, was found to be a portion of placenta. On its extraction every disagreeable symptom disappeared. (That the cause of the obstinate vomiting was reflex irritation, having its starting-point in the uterus, seems without doubt. Was this irritation before delivery set up by the peculiar nature of the presentation? Had the feet descended into contact with the cervix uteri in consequence of conversion from an originally different presentation?)

III. Labour.

1. *On a Case of Artificial Premature Labour by Scanzoni's Method.* By Spaeth. (Wochenbl. d. Zeitschr. der Ges. d. Aerzte zu Wien. 1856.)
2. *A Case of Inversion of the Uterus.* By Professor White. (Buffalo Med. Journ. March, 1856.)
3. *Case of Inversio Uteri.* By Samuel P. Brown, M.D., of Greensburgh, Pa. (Medical Examiner. June, 1856.)
4. *The Numerical Relations of the Occurrence of Placenta Prævia in Kurhessen.* By Dr. Schwarz, of Fulda. (Monatsschr. f. Geburtsk. August, 1856.)
5. *Extra-uterine Gestation outside the Abdominal Cavity.* By Dr. Genth, of Schwalbach. (Verhandl. der Ges. für Geburtsk., Berlin, 1855.)

1. (In a former Report the method of Scanzoni for inducing premature labour was described. The following note contains the results of its application in practice.)

In Spaeth's case the application of suction to the breasts brought on strong pains, which exerted a marked influence on the cervix uteri, but had to be intermitted on account of the disagreeable effect upon the breasts. Out of eight cases in which the method has been tried, it succeeded in four. In the quickest, labour followed in seven hours; in the slowest, on the thirteenth day after twenty-one applications. The result depends partly on the reflex excitability of the individual, partly on the condition of the nipples. One advantage attending it is the freedom from hurtful influence on the fœtus.

2. Professor White's patient had been attended in her labour by a German midwife, who stated that after a brief labour she had given birth to a male infant weighing upwards of ten pounds. The placenta very soon came away, accompanied by the inverted uterus, which descended into the vagina. The flooding at this time was described as terrific, and caused protracted syncope. No medical assistance was requested for several days afterwards, by which time the tumour had descended through the os externum. The first attempt to replace it was unsuccessful, in consequence of the fainting of the patient from loss of blood during the effort. Next day (the eighth after accident), with the aid of chloroform, a more successful attempt was made. The dimpling of the fundus was preserved by a rectum bougie, whilst the hand was allowed to rest. The re-position was effected by great perseverance under great difficulty. The hæmorrhage ceased entirely after replacement. The patient, however, died two days afterwards. Autopsy revealed great anæmia. There were slight marks of inflammation with exudation, in the peritoneum. The uterus exhibited nothing abnormal.

3. The case of inversion of the uterus related by Dr. Brown serves to illustrate the mode of occurrence of this accident. Mrs. E., a healthy young woman, in her second confinement, was in labour at three A.M. on the 3rd of April, 1856. The pains were recurring, at intervals of fifteen minutes, short, and without trismus. At eight A.M. they became more frequent and effective. Whilst walking about the room a strong bearing-down came on, during which she got on her knees close by the bed; with a violent expulsive effort the child was expelled. She immediately complained of exhaustion, and pain in the abdomen, and desired to be put to bed. In a few minutes after she was found to be flooding profusely. Whilst Dr. Brown's hand was on the abdomen she was seized with a violent pain, which he thought expelled the placenta, but on introducing his hand he found the uterus inverted and the placenta adherent. Dr. Brown attempted the re-position of the uterus with the placenta attached, but failed. Shortly after Dr. R. Brown peeled off the placenta, grasped the uterus in his hand, reintroduced it into the vagina, carried it through the os uteri, and, indenting the fundus, pushed it forwards with the fingers in a conical shape, and thus without much difficulty replaced it. Strong uterine contractions followed, and on examination the fundus was found again slightly depressed. The patient rallied and did well.

4. Those statistical summaries which are based upon complete observation, are clearly the only ones which can furnish laws. In the district of Kurhessen, *every* delivery is recorded, and the nature of the labour related, by the medical attendant. We thus possess the necessary elements for estimating the comparative frequency of particular obstetric occurrences. In the course of twenty years, there were 519,328 births, and placenta prævia occurred 332 times. The numbers vary remarkably in different years—namely, from 8, the lowest number, to 76, the highest. Of these 332 cases, 246 women recovered; 86 died. 251 children were still-born; 85 lived. 40 women were primiparæ. Turning by the foot was practised in 259, and in 7 turning by the hand; 23 children had to be extracted by forceps; 13 were extracted by Cæsarean section after death of mother. The placenta was artificially removed 8 times, and 16 times plugging was resorted to.

5. The case of Dr. Genth is of a very unusual kind. A woman, aged thirty-four, had borne from her earliest childhood an easily-moveable oval tumour, the size of a hazel-nut, in the left side, near the external abdominal ring, under the skin. This tumour gradually reached the size of a walnut after the establishment of menstruation. It was always tender on pressure. At twenty-four she married, and fell pregnant immediately. Without known cause, she suffered in the fourth month a prolapsus uteri, which towards the end of pregnancy had attained the size of a child's head. The tumour in the side remained unchanged. The labour was normal. The prolapsus continued, and had to be supported by a band. After

some years another pregnancy, with prolapsus uteri, took place, without any influence upon the tumour in the side. The third pregnancy resembled the preceding, and ended in 1850. She menstruated regularly until February, 1852.

After menstruating as usual, on the 22nd of February, 1852, the catamenia ceased. Some weeks later, the patient perceived an enlargement of the tumour in the side; it became painful and inflamed; the inflammation was removed by local antiphlogosis; but the tumour grew rapidly. Sixteen and a-half weeks from the cessation of the catamenia the tumour was as big as two fists, uniformly tense, so that its contents could not be determined. It had extended under the skin to the labium majus. In the inguinal canal was felt a pedunculated prolongation, of the thickness of the little finger. The tumour was so distinct from the abdominal cavity, that the horizontal branch of the pubes could be felt between. The patient was now very weak, anæmic, and forced to keep her bed. She begged earnestly for the removal of the tumour. An exploratory puncture was made; some ounces of water followed, and then a quantity of arterial blood. The tumour lost nothing of its tension, and so its contents could not be made out. A longitudinal incision was made. Under the outer coverings a tendinous membrane was seen, which being slit, a capacious round cavity was exposed. In this the finger detected an easily moveable fœtus, which lived for a moment after extraction. It was about four or five months old, and well-formed. The placenta was implanted all round the cavity, and the greater part was removed by the forefinger, with much loss of blood. The wound was closed, and hæmorrhage stopped by cold. On the seventh day another piece of the placenta was removed, which had occasioned another bleeding. In three weeks the wound had almost entirely closed. She recovered her strength quickly. After complete cicatrisation of the wound, the tumour was felt, as before, easily moveable under the skin.

The woman has had a natural pregnancy since. Dr. Genth has no doubt that the tumour was formed by the ovary, which, like the testicle, had escaped from the abdominal cavity, drawing the Fallopian tube after it. The passage of the impregnated ovum into the abdomen was somehow stopped, perhaps by compression of the abdominal muscles, and thus the development of the ovum took place not only outside the uterus, but even outside the abdomen. The falling of the uterus at each gestation is explained by the dragging of the ligamentous part of the ovary preventing the uterus from rising up towards the thorax.

[The Reporter would remark that a stethoscopic examination of the tumour would have revealed the presence of a living fœtus. It is also a matter of question whether removal of the entire tumour, including the ovary, would not have been proper, in order to prevent the possible recurrence of this hernial gestation.]

IV. Puerperal and Post-Puerperal Diseases.

1. *Account of a Puerperal Epidemic raging concurrently with Cholera in the Lying-in Hospital at Trient.* By Professor Braun. (Zeitschr. der k. k. Ges. d. Aerzte zu Wien. Aug. 1856.)
2. *Remarks on the Consecutive Diseases of the Puerperal Condition.* By Dr. Mikschik of Vienna. (Zeitschr. der k. k. Ges. d. Aerzte zu Wien. Nos. 3 and 4. 1856.)

1. We extract from Professor Braun's Report of the movement of the Trient Lying-in and Foundling Hospital for 1855, some brief notes relating to the spread of a form of puerperal fever during the prevalence of cholera. During the year 1855, 279 pregnant women were admitted, 40 remained over from the preceding year, so that 319 were treated. 30 went out undelivered. Of the women delivered, 191 were discharged well; 66 were transferred to the Poor-house, 6 to the Trient Hospital, and 12 died; some remained undelivered. The mortality of the puerperal women was as 1 to 28, or 3·5 per cent. Twenty per cent. of those delivered in August died, whereupon the most speedy separation of the sick from the healthy was effected. With the exception of August, the health-condition of

the house was good. There died in February, 1 woman of asthma of the lungs; in March and May, in each, 1 of pleuritis and peritoneal exudations; in June, 1 of apoplexy of the brain; but in August, when the cholera epidemic had reached its culmination-point in the town, 6 died of a puerperal disease. In this summer, out of a population of 330,000, more than 6200, or 1·9 per cent., were carried off by cholera. From the 10th to the 18th August, 8 women out of 12 delivered, or one daily, sickened, of whom 5 died within a few days. On the 16th August, a man belonging to the house fell ill of cholera, and died on the 20th. Two days after, a perfectly healthy woman, who had been delivered nine days, took the cholera, and was transferred on the same day into the Cholera Lazaretto. Concurrently with this intense puerperal-fever epidemic, cholera infantum spread amongst the children. There appears to have been a serious want of water; a great terror seized the pregnant women; in consequence, a temporary hospital was procured, to which some of the patients were removed, whilst others were sent to their homes, and no more were admitted, except extreme cases, until the 30th September. The first child died of cholera infantum on the 16th August, and the last on the 12th September. The wards were cleansed by chlorine gas and heating, after Busch's method, and fourteen days later, re-occupied without ill consequence.

The following summary of the cases gives a general view of the origin, cause, and symptoms of the fever:—

No.	Day of delivery.	Day of sickening.	Result.	Remarks.
159	... Aug. 10th	... Aug. 10th	... Aug. 12th, died	... Strong fever, meteorism, diarrhœa and vomiting.
178	... Aug. 10th	... Aug. 12th	... Aug. 16th, died	... Peritoneal exudation, vomiting, and diarrhœa.
175	... Aug. 6th	... Aug. 12th	... Aug. 22rd, died	... Septicæmia without localization; no diarrhœa or vomiting.
183	... July 30th	... Aug. 12th	... Aug. 20th, recov.	... Metrorrhagia, continued fever: quinine.
151	... Aug. 12th	... Aug. 15th	... Aug. 19th, died	... Septicæmia, no vomiting, diarrhœa, or meteorism.
171	... Aug. 14th	... Aug. 16th	... Sept. 19th, recov.	... Perimetritis, Aug. 28th: miliaria, Sept. 3rd; metastatic abscesses.
140	... July 29th	... Aug. 16th	... Sept. 22nd, died	... Metrorrhagia, then pleuritic exudations.
105	... Aug. 15th	... Aug. 18th	... Oct. 18th, died	... Septicæmia, Sept. 1st, choleraic diarrhœa and metastasis. Autopsy: large purulent deposit in apex of right lung.

It was remarked that the cases of puerperal fever preceded those of cholera. It is in the highest degree probable that this puerperal fever arose out of the influences and relations with the ruling contagious miasmatic diseases; for since the 21st July, no autopsy was performed by either of the attending physicians; and after July, every patient on admission was supplied with clean linen, and her own clothing purified by chlorine gas for twenty-four hours. External communication was cut off; the school was closed on 15th July; no obstetric operations were called for in this period; and several pounds of sulphate of iron were daily thrown into the privies. The deficiency of water was supplied at great cost, and good diet (*without vegetables and fruit*) allowed.

During the winter of 1856, no case of puerperal fever appeared.

2. Dr. Mikschik refers to the labours of Heschl, Rokitansky, Wedl, Virchow, Retzius, Rainey, &c., on the fatty transformation of the uterine tissues after labour, preparatory to the return of this organ to its normal state. He observes that, when unfavourable circumstances arrest this transformation, the womb remains in the state of fatty degeneration, and will be found either increased or diminished in volume. This latter condition, more rare than the first, was noted in 1842 by Rokitansky, who pointed out a remarkable brittleness of the uterine fibre. The organ remains porous, pale red, of a slaty-grey here and there, yellowish, and pale yellow. Amongst the causes which induce these changes of volume of the uterus, must in chief be noted puerperal diseases, which fill the organ with exudation-liquids, and envelope it and its appendages with thick

exuded masses. The causes next in frequency are tubercular diseases; but of 16 cases, Mikschik found the volume of the uterus increased in 13. In the 3 cases of atrophy, the connexion was with tuberculous disease. The time that had elapsed between delivery and death was from ten weeks to two years.

Can a fatty uterus conceive, and become the seat of menstruation? This question cannot be answered positively. But the possibility is doubtful; for autopsies have shown that, in intense puerperal affections, the exudations are deposited, not in the uterus alone, but in the ovaries also, and that the Graafian follicles are thus often destroyed. But there exists sometimes a sanguineous discharge that may be mistaken for menstruation: this is because the vessels of a fatty uterus remain open, and the blood is able to escape the more easily from the friable condition of the capillaries. Later, when the fatty metamorphosis has made progress, the researches of Wedl have shown that the vascular walls and the small capillaries which have no *vasa vasorum*, disappear for want of nutrition. Then results a local anæmia which dries up the source of hæmorrhage. The atrophied womb causes no disorder; but since amenorrhœa is the inevitable consequence, and since the primitive lesion is always accompanied by other alterations, the patients seek for the cause of their illness in the absence of menstruation, and torment the physician to prescribe in this direction. It is therefore necessary to be on our guard not to do mischief, in trying to restore impossible menstruation.

Amongst the other consecutive diseases, the most frequent and the most severe are the consequence of peritonitis. This easily and frequently returns. Each one of these forms of partial peritonitis has a course and gravity which depend upon the nature, quantity, and situation of the exudations caused by the primitive disease. Such are the deposits in the pelvis, around the womb; the transformation of the exudation into tubercles; its organization into false membranes, bands, which determine the most various modifications in the situation and relations of the abdominal organs; the shortening of the mesentery; incurable displacements with fixing of the womb, dragging the bladder and rectum in the empty state of the womb, and giving rise to more severe accidents in the event of pregnancy; lastly, all the disorders which result from disturbance of the functions of the intestines, and the sterility following upon a faulty position of the Fallopian tubes.

MEDICAL INTELLIGENCE.

The Imperial Society of Medicine of Constantinople.

WHATEVER may be the ultimate results of the late war, on the civilization of the Turkish Empire, it appears that medical science and the state of the profession are likely to be advanced and benefited.

Nowhere is such a motley crowd of medical practitioners (qualified and non-qualified) to be found as in Constantinople: English, French, Italians, Greeks, Armenians, Jews, Turks, &c., may fairly be said to represent every system that has ever been propounded in medicine. Though it had been long felt that it would be of the greatest interest to bring all these different physicians together, and of great advantage in promoting a good understanding among them, the scheme of forming a Medical Society, though sometimes contemplated, could never be carried out, owing to the jealousy of so many conflicting nationalities, and the local difficulties of habitation and practice.

It having struck Dr. Pincoffs, one of the physicians of the Scutari hospitals, that the circumstances which during the late war brought an additional influx of medical officers, offered peculiar opportunities for carrying out such an object, he succeeded, on the 13th of February, 1856, in effecting a meeting of the chief medical officers of the English, French, and Sardinian military hospitals, and of the leading physicians of Constantinople; they then resolved that they would assemble regularly so long as the war should last, with a view of ultimately establishing a permanent Medical Society at Constantinople.

M. Baudens, Inspector-General of the French hospitals in the East, was elected President; Dr. Linton, Inspector-General of the Scutari hospitals, and Dr. Fauvel, Physician to the French Embassy and Médecin Sanitaire de France, Vice-Presidents of the Society.

The Society have ever since met regularly every fortnight; the all-engrossing feature of the French hospitals—typhus fever—has formed the topic of the principal papers and discussions; and, if we say that physicians of all nations, and belonging to all schools, have spoken on the subject, we are sure that our readers will look forward with interest to a detailed account of the proceedings, shortly to be published by the Society itself, and which will contain a vast amount of individual observation. We take from the 'Union Médicale (Procès Verbaux de la Société Impériale de Méd. de Const., Juin—Septre. 1856), the names of the different medical men who have read papers and have spoken at the meetings:—Of the French Military Hospitals, Doctors Cazalas, Thomas, Nettes, Jacquot, Valette, Pastureau, Barudel, Baudens, Quesnoi, Garreau, and Grellois; of the Naval Hospital: D. Arnaud; of the English Hospitals: Drs. Dryce, Freund, Pincoffs, Temple; of the Turkish Army: Dr. Bonelli; of the Constantinople Physicians: Drs. Fauvel, Caratheodory, Sotto, Pardo. The names of two Russian physicians, sent by their Government to observe the typhus fever in various localities, are now also found: Drs. Alferieff and Moering.

Since the departure of the allied armies, Dr. Fauvel, a man of acknowledged scientific and professional standing, has been the President of the Society; the Council is formed of some of the principal physicians of Constantinople, and the Society numbers above forty resident, and several honorary and corresponding, members. One of the chief statutes of the Society, that none but properly qualified practitioners are to be members, will contribute not a little to raising the standard of the profession. The Society has been fortunate enough to obtain the patronage of Fuad Pasha, the Minister for Foreign Affairs. This distinguished statesman, who has the most enlightened views on all matters, is fully able to appreciate the benefits likely to result from such an association for Turkey, the more so as he has himself studied medicine; he has accepted the title of honorary member, and procured from the Sultan a *Bérat*, granting to the Society the title and privileges of an Imperial Society of Medicine (*Djemiéti Thebiei Chahané*). It is also probable that the Society will obtain a considerable pecuniary grant from Government; and it intends publishing a periodical account of its transactions.

The interest of such a publication will be all the greater, from the circumstance that the members of the *conseil de santé* (board of health) are members of the Society. This board has paid medical correspondents throughout the Levant, and as far as the borders of Persia and India, it will thus be easily understood that much valuable information on those countries (involving questions of epidemics, quarantine, and other matters of scientific and commercial importance) may be imparted and diffused by this channel.

Edible Seaweeds.

Some very interesting experiments were published in the July number of the 'Edinburgh New Philosophical Journal,' by Dr. John Davy, on the constitution of certain edible seaweeds,—the Chondrus crispus, the Rhodomenia palmata, Porphyra lacunata, Laminaria digitata, and Fucus vesiculosus. The main result was the determination of the presence of a larger quantity of nitrogen than is contained in the best flour. We merely allude to Dr. Davy's observations, that we may draw attention to the fact that two prizes of 50*l.* and 20*l.* respectively have been offered by Sir C. Trevelyan, Bart., for the best essays on the applications of the marine algæ and their products as food or medicine for man and domestic animals. We regret that we have not space to do more than advert thus briefly to the matter.

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Lectures on the Principles and Methods of Medical Observation and Research. By Thos. Laycock, M.D. Edinburgh, 1856. pp. 218.

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The True Art of Healing. A Discourse on the Rational System of Medicine. Delivered Oct. 1, 1856. By Thomas K. Chambers, M.D., &c. London, 1856. pp. 32.

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AND MEDICAL COLLEGE.

THE WINTER SESSION will commence on the 1st of OCTOBER, with an INTRODUCTORY ADDRESS by Mr. M'WHINNIE, at Seven o'clock, P.M.

LECTURES.

Medicine	Dr. BURROWS and Dr. BALY.
Surgery	Mr. LAWRENCE.
Descriptive Anatomy	Mr. SKEY.
Physiology and Morbid Anatomy	Mr. PAGET.
Chemistry	Dr. STENHOUSE.
Superintendence of Dissections	Mr. HOLDEN and Mr. SAVORY.

SUMMER SESSION, 1857.—COMMENCING MAY 1.

Materia Medica	Dr. F. FARRE.
Botany	Dr. KIRKES.
Forensic Medicine	Dr. BLACK.
Midwifery, &c.	Dr. WEST.
Comparative Anatomy	Mr. M'WHINNIE.
Practical Chemistry	Dr. STENHOUSE.

HOSPITAL PRACTICE.

The Hospital contains 650 Beds, and relief is afforded to more than 90,000 Patients annually. The In-patients are visited daily by the Physicians and Surgeons; and Clinical Lectures are delivered weekly,—on the Medical Cases by Dr. BURROWS and Dr. FARRE; on the Surgical Cases by Mr. LAWRENCE, Mr. STANLEY, Mr. LLOYD, and Mr. SKEY; on Diseases of Women by Dr. WEST. The Out-patients are attended daily by the Assistant-Physicians and Assistant-Surgeons.

COLLEGIATE ESTABLISHMENT.

Students can reside within the Hospital Walls, subject to the rules of the Collegiate System, established under the direction of the Treasurer and a Committee of Governors of the Hospital. Some of the Teachers and other Gentlemen connected with the Hospital also receive Students to reside with them.

SCHOLARSHIPS, PRIZES, &c.

At the end of the Winter Session, Examinations will be held for two Scholarships of the value of £45, for a year. The Examination of the Classes for Prizes and Certificates of Merit will take place at the same time.

Further Information may be obtained from MR. PAGET, MR. HOLDEN, *or any of the Medical or Surgical Officers or Lecturers, or at the Anatomical Museum, or Library.*

A 2

UNIVERSITY COLLEGE, LONDON.

FACULTY OF MEDICINE.

SESSION 1856-57.

THE CLASSES will COMMENCE on WEDNESDAY, OCTOBER 1st.

Classes in the order in which Lectures are delivered during the day:—

WINTER TERM.

Anatomy	Professor ELLIS.
Anatomy and Physiology	Professor SHARPEY, M.D., F.R.S.
Chemistry	Professor WILLIAMSON, F.R.S.
Comparative Anatomy	Professor GRANT, M.D., F.R.S.
Surgery	Professor ERICHSEN.
Medicine	Professor WALSHE, M.D.
Dental Surgery	HUBERT SHELLEY, M.B.

Practical Anatomy.—The Pupils will be directed in their studies during several hours daily by Professor ELLIS and Mr. DAVID B. REID, Demonstrator.

SUMMER TERM.

Botany	Professor LINDLEY, Ph.D., F.R.S.
Pathological Anatomy	Professor JENNER, M.D.
Forensic Medicine	Professor CARPENTER, M.D., F.R.S.
Practical Chemistry	Professor WILLIAMSON, F.R.S.
Midwifery	Professor MURPHY, M.D.
Palæo-Zoology	Professor GRANT, M.D., F.R.S.
Ophthalmic Medicine and Surgery	Professor T. W. JONES, F.R.S.
Materia Medica	Professor GARROD, M.D.
Practical Physiology and Histology	Teacher, Dr. G. Harley.

Analytical Chemistry . . . . Prof. WILLIAMSON, throughout the Session.

Logic, French and German Languages, Natural Philosophy, Geology, and Mineralogy—according to announcement for the Faculty of Arts.

CLINICAL INSTRUCTION.

Hospital Practice daily throughout the Year.

Physicians	Dr. WALSHE, Dr. PARKES, Dr. GARROD, Dr. JENNER.
Obstetric Physician	Dr. MURPHY.
Assistant-Physician	Dr. HARE.
Surgeons	Mr. QUAIN, Mr. ERICHSEN.
Consulting Surgeon to the Eye Infirmary	Mr. QUAIN, F.R.S.
Ophthalmic Surgeon	Mr. WHARTON JONES.
Assistant-Surgeons	Mr. MARSHALL, Mr. HENRY THOMPSON.
Dental Surgeon	Mr. SHELLEY.

Medical Clinical Lectures by Dr. WALSHE, Dr. GARROD, and Dr. MURPHY; also by Dr. PARKES, Professor of Clinical Medicine, whose special duty it is to train the Pupils in the Practical Study of Disease, and who gives a Series of Lessons and Examinations on the Physical Phenomena and Diagnosis of Disease to Classes consisting of a limited number, and meeting at separate hours.

Surgical Clinical Lectures, specially by Mr. QUAIN and by Mr. ERICHSEN.

Lectures on Ophthalmic Cases, by Mr. WHARTON JONES.

Practical Instruction in the Application of Bandages and other Surgical Apparatus, by Mr. MARSHALL.

Practical Pharmacy.—Pupils are instructed in the Hospital Dispensary.

Prospectuses may be obtained at the Office of the College.

RESIDENCE OF STUDENTS.—Several of the Professors receive Students to reside with them; and in the Office of the College there is kept a Register of parties unconnected with the College who receive Boarders into their families. Amongst these are several medical gentlemen. The Register will afford information as to terms and other particulars.

WM. JENNER, Dean of the Faculty.

August, 1856. CHAS. C. ATKINSON, Secretary to the Council.

The Lectures to the Classes of the Faculty of Arts will commence on Tuesday, the 14th of October.

UNIVERSITY AND KING'S COLLEGE, ABERDEEN.

MEDICAL SCHOOL.

THE WINTER SESSION commences on the FIRST MONDAY of NOVEMBER, and terminates on the THIRD FRIDAY of APRIL. INTRODUCTORY LECTURE on the FIRST MONDAY of NOVEMBER, at Two o'clock P.M.

SUBJECTS.	LECTURERS.	HOURS.	FEES.		
Midwifery	Dr. RAINY	8 to 9 A.M.	3	3	0
Anatomical Demonstrations	Dr. REDFERN	12 to 1 P.M.	2	2	0
Chemistry	Dr. FYFE	$10\frac{3}{4}$ to $11\frac{3}{4}$ A.M.	3	5	6
Materia Medica	Dr. RATTRAY	9 to 10 A.M.	3	3	0
Practice of Medicine	Dr. WILLIAMSON	1 to 2 P.M.	3	3	0
Anatomy and Physiology	Dr. REDFERN	2 to 3 P.M.	3	3	0
Surgery	Dr. KERR	3 to 4 P.M.	3	3	0
Institutes of Medicine (Physiology)	Dr. CHRISTIE	4 to 5 P.M.	3	3	0

SUMMER SESSION.

Botany	Rev. J. C. BROWN	8 to 9 A.M.	2	2	0
Practical Chemistry	Dr. FYFE	12 to 1 P.M.	3	5	6
Anatomical Demonstrations	Dr. REDFERN	2 to 3 P.M.	2	2	0
Medical Jurisprudence	Dr. REID	3 to 4 P.M.	2	2	0
Histology	Dr. REDFERN.				
Natural Philosophy	DAVID THOMSON, M.A.		2	2	0
Natural History	Rev. J. LONGMUIR.				

The Rooms for Practical Anatomy are personally superintended by Dr. Redfern.

Students are required to matriculate within the first month of the Winter Session, and within the first fortnight of the Summer Session, and no certificate of attendance will be given without such matriculation. The Matriculation Fee for all the Classes is one sum of 5s. for the Winter, and one of 2s. 6d. for the Summer Session.

CHEMISTRY will be taught, as formerly, in King's College; and the other Classes in the Medical School, St. Paul's-street.

ROYAL INFIRMARY.

The Hospital is open daily at 10 o'clock A.M., and contains upwards of 300 beds. Separate Courses on CLINICAL MEDICINE and CLINICAL SURGERY are delivered in the Hospital twice a-week.

Physicians—Dr. DYCE, Dr. W. WILLIAMSON, Dr. KILGOUR, and Dr. NICOL.

Surgeons—W. KEITH, Esq.; D. KERR, Esq.; W. PIRRIE, Esq.; and A. J. LIZARS, Esq.

Ophthalmic Surgeon—JOHN CADENHEAD, Esq.

Lecturers on Clinical Medicine and Surgery—Dr. KILGOUR and W. KEITH, Esq.,

Fee for the Medical and Surgical Practice of the Hospital, first year, £3 10s.; second year, making perpetual, £3; or one sum of £6.

CLINICAL MEDICINE—for the first Course, £2 2s.; subsequent Courses, £1 1s. each; perpetual, £4 4s.

CLINICAL SURGERY—for the first Course, £2 2s.; subsequent Courses, £1 1s. each; perpetual, £4 4s.

DISPENSARY.

The ABERDEEN GENERAL DISPENSARY, VACCINE, and LYING-IN INSTITUTION, is open to the Student on application to the Medical Officers. There are annually about 5000 patients, either prescribed for at the Institution or visited at their own houses.

LUNATIC ASYLUM.

The ASYLUM contains about 240 patients. A limited number of Students are admitted to see the practice. Fee, for a Course of Three Months, £2 2s.

Consulting Physician—Dr. MACROBIN.

Resident Physician and Superintendent.—Dr. ROBERT JAMIESON.

The Library of the University contains about 55,000 volumes, and is free to all the Students of KING'S COLLEGE.

The Medico-Chirurgical Society possesses a valuable Medical Library of nearly 4000 volumes, and a Reading-Room, to both of which, and to all the privileges of Junior Members, Students are admitted on payment of £1 1s. per year, becoming free members after three payments.

DAVID THOMSON,
Sub-Principal and Secretary.

King's College, Aberdeen, Sept. 1856.

LIVERPOOL ROYAL INFIRMARY SCHOOL OF MEDICINE.

THE SESSION of 1856-7 will COMMENCE on WEDNESDAY, 1st OCTOBER, when an INTRODUCTORY LECTURE will be given at Two o'clock by T. F. GRIMSDALE, Esq.

WINTER SESSION.

	Fee for one course.		
Principles and Practice of Surgery—Mr. LONG, F.R.C.S.E., and Surgeon to the Royal Infirmary	£4	4	0
Principles and Practice of Physic—THOMAS INMAN, M.D., Physician to the Northern Hospital	5	5	0
Anatomy, Physiology, and Pathology—Mr. F. D. FLETCHER and Mr. A. T. H. WATERS, Surgeons to the Dispensaries; *Anatomy, Descriptive & Surgical*	8	8	0
Chemistry—J. B. EDWARDS, Ph.D.	5	5	0

SUMMER SESSION.

Midwifery & Diseases of Women—Mr. BATTY, Cons. Surg. to Ladies' Charity; *Diseases of Children*—Mr. GRIMSDALE, Surgeon to the Lying-in Hospital	4	4	0
Materia Medica & Therapeutics—J. BIRKBECK NEVINS, M.D. Lond., F.R.S.	4	4	0
Medical Jurisprudence—J. CAMERON, M.D., Physician to the Southern Hospital, and Dr. EDWARDS	3	3	0
Botany—Mr. T. C. ARCHER	3	3	0
Ophthalmic Medicine and Surgery—R. HIBBERT TAYLOR, M.D., Surgeon to the Eye and Ear Infirmary	1	1	0
Practical Chemistry—Dr. EDWARDS	3	3	0

The Dissecting Rome is open daily from 8 A.M. to 6 P.M. The Students are superintended in their Dissections by the Anatomical Lecturers.

ANNUAL EXHIBITIONS.—*Royal Infirmary Medical Scholarship*—value, 42*l.*—consisting of a Gold Medal, value, 10*l.* 10*s.*, and six months' free board and residence, with Dressership and Clerkship in the Royal Infirmary. In case the Scholarship is gained by a resident Pupil, six months' payment (31*l.* 10*s.*) will be returned to him.

Four Exhibitions—value 31*l.* 10*s.* each—consisting of free board and residence in the Royal Infirmary for six months—with Dressership on award of the Medical Board.

Medals and other Prizes are awarded annually in each subject at the close of the Session.

HOSPITAL PRACTICE.—Students are admitted to the Medical and Surgical Practice of the Liverpool Royal Infirmary from 10 A.M. to 1 P.M. daily. The Certificates are received by the University of London, the Royal College of Physicians of London, the Royal College of Surgeons of England, the Worshipful Society of Apothecaries, and the Army Board.

The Liverpool Royal Infirmary contains 226 Beds, Operating, and Lecture Theatres; and a good Museum of Healthy and Morbid Anatomy is attached to the School.

Six Dressers and Six Clinical Clerks will be elected quarterly, by selection and examination, from amongst the House and Out-Pupils of the Infirmary, from the Students attending the School in their Second and Third Session, and from all Medical Pupils who have entered on their third year of professional education. The Office will be tenable for two successive periods. Pupils of the Infirmary are admitted to learn Pharmacy in the shop for six months. Clinical wards have been set aside for select cases, and a Clinical Prize instituted for annual competition.

FEES.—*Lectures.*—For all the Lectures (*including* Practical Chemistry) required by the Hall and College, 45*l.*, payable in advance.

Medical and Surgical Practice.—For Six Months, 10*l.* 10*s.*; for the First Year, 18*l.* 18*s.*; for the Second, 12*l.* 12*s.*; for the Third, 10*l.* 10*s.*; for Three Years, 36*l.* 15*s.* *No additional fees whatever.*

INFIRMARY APPRENTICES.—Four Apprentices are admitted for five years, to reside and board in the house, for 60 guineas per annum; and for one, two, or three years, upon payment of 70, 130, or 190 guineas. These sums include Library and *Lecture* Fees, but not Hospital Practice.

REGISTRATION.—The Register of Tickets of Admission to Lectures or Hospital Practice is open from October 1st to 14th, 1856, and from May 1st to 7th, 1857, the latter day inclusive. Application to be made to Dr. NEVINS, at his residence, 25, Oxford-street.

Liverpool Royal Infirmary School of Medicine, September, 1856.

TESTIMONIALS
TO
DR. JOSEPH BULLAR & DR. WILLIAM BULLAR,
OF
SOUTHAMPTON.

A SUBSCRIPTION of upwards of £550 having been raised by the inhabitants of Southampton and its neighbourhood, for the purpose of presenting a Testimonial to these gentlemen, in token of the esteem in which they are held, as well for their private worth, as for the valuable services rendered by them to the Royal South Hants Infirmary, the one as Physician, the other as Honorary Secretary, since its formation in 1844, a deputation, consisting of the following gentlemen, namely,—

Colonel EYRE CRABBE, K.H.,
Archdeacon WIGRAM,
Rev. THOMAS ADKINS,
Captain ENGLEDUE, and
Mr. H. J. PINSENT, Hon. Secretary,

waited upon the Doctors BULLAR, at their residence, a few days since, to present the Testimonials, which consisted of two massive Silver Salvers, valued at One Hundred Guineas each, and a Purse containing Three Hundred Sovereigns.

The following is a copy of the inscription on each Salver, and is contained in a central compartment, surrounded by scroll work, six other compartments containing the names of the whole of the subscribers: —

"To JOSEPH BULLAR, Esq., M.D., and WILLIAM BULLAR, Esq., M.D., from their friends and fellow-townsmen, two Salvers, with 300 Sovereigns, are presented, in testimony of the eminent professional ability with which they have aided the Royal South Hants Infirmary since its formation in 1844, Dr. WILLIAM BULLAR having gratuitously fulfilled the arduous duties of Secretary during the whole of that period; and also of the genuine benevolence and Christian disinterestedness with which, at all times, and among all classes, these attached brothers have laboured to promote the charities of social life, and to advance the temporal and spiritual interests of this their native town. 1856."

New Burlington Street, London,
1st October, 1856.

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